THE AMERICAN
JOURNAL OF PSYCHOLOGY

Founded by G. STANLEY HALL in 1887.

VOL. XV. OCTOBER, 1904. No. 4.

THE PSYCHOLOGY OF DAY DREAMS.

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Perhaps not the least difficult question in connection with the present topic is what mental states shall be included under the term day dreaming. The usual definition "as an idle exercise of the imagination during waking hours," by no means covers the material of the returns, which include nearly every form of mental reproduction from the hypnagogic state, with complete absence of voluntary control, through varying phases in which the initial idea or general trend of the images is voluntarily determined, up to a distinctly purposive picturing of the future with due attention to probable realization. There are, however, certain characteristics which are common to this entire series of phenomena, namely a withdrawal of the attention, more or less complete, from the external senses, and a greater or less degree of mental automatism. Fechner[1] considers that in so far as attention is withdrawn from the senses, their condition is precisely the same as in real sleep and "*vice versa* the whole sphere of the activity of inner representations may fall asleep." According to this view, the mental life oscillates between sleeping and waking and there are regions of the brain asleep even in waking states and the distinction between dreams and day dreams is merely one of degree. For convenience in classification, day dreaming may be tentatively defined as including all those reproductive and imaginative mental states in which there is a greater or less degree of automatism in the images which come before the mind. Its limits would be, on the one hand, the hypnagogic states which immediately precede sleep and on the other, states of purposive

[1] G. T. Fechner: Elemente der Psychophysik. Leipzig, 1889, p. 440.

thinking in which the mind becomes so filled with the subject that its workings tend to become automatic. Some of those who answered the questions attempted definitions, a few specimens of which are here given.

F., 14. Day dreams are the thoughts and wishes which we imagine.
M., 15. Day dreams are dreams about things which are fancied and which have no real foundation.
F., 16. Day dreams are thoughts about what we want the most.
F., 16½. Day dreaming is simply the soul longing for something great.
F., 13. In day dreams, you first start out to think about one thing and then your mind wanders over many things which may or may not be connected with what you first start to think about. It is really going to sleep because you don't work with anything but your brain. You generally have day dreams when everything about you is quiet and you have nobody to talk to. You think, but you don't express your thoughts in words, its your brain that is holding a conversation.

The material for the present study was collected in response to a request contained in a syllabus on dreams.

"Ask all who can to write about their day dreams, what they are most often about, and describe one or more in detail; where and when they lapse to revery most often, and if they enjoy it or think it wrong, etc."

469 papers were received from normal school pupils, ages averaging from seventeen to twenty-five. 980 from pupils in the graded schools of ages ranging from seven to sixteen years. 23 from adults and three contributions were received from those who had passed the age of ninety, making a total of 1,475 cases. Of these 535 were from girls and 445 from boys in the graded schools. The normal school material was chiefly from girls, and of the adults slightly over one half were men. Among the entire 1,475 cases there were five (3 m. and 2 f.) who stated positively that they never had day dreams but of these five one, a man of twenty-five, described a mental state which would be included in the broader definition of day dreaming and two others were children who were classed in grades with children several years younger.

The physical characteristics of day dreaming most frequently mentioned were psychic deafness and blindness and muscular relaxation, including that of the eyes. Many children give descriptions of individual instances of this psychic deafness. They fail to hear bells or signals, say that their minds were far away, that the teacher had to speak several times to attract their attention, and one boy graphically describes an occasion on which his teacher threw a piece of chalk at him "to wake him up." A girl of twenty-one writes: "Sometimes I am so interested in my dreams that I do not see or hear anything that is going on around me." Another, of seventeen: "There are times when I

am so far away that I am entirely unconscious of my surroundings until there is some loud noise or my name is called." The "far away" look of the eyes is repeatedly mentioned and is an external sign of day dreaming which children readily recognize. A child of twelve gives her observations on the difference between hard thinking and day dreaming in these words: "When you do your arithmetic you pucker up your forehead, but when you are day dreaming your eyes look way off." This relaxation of the eye muscles which allows the axes to become parallel, or according to Donders, actually divergent, is similar to that in actual sleep. Le Conte proved experimentally that in drowsiness and drunkenness the double images are due to divergence of the optic axes and recognizes this as the absolutely involuntary and passive state of the eye in distinction from the involuntary tonic contraction of the healthy waking state which holds the lines of regard parallel, and the voluntary state of convergence.[1] It seems probable that both of the involuntary states are represented in day dreaming, actual divergence of the axes probably being confined to those mental states which most nearly approach the hypnagogic while the "far away" look so often mentioned is due to parallel axes. Besides these cases of more or less complete muscular relaxation there is another class in which day dreaming is an accompaniment of physical activity of a monotonous or automatic character, as walking, sewing, driving, swinging in a hammock, rocking, practicing piano exercises, hoeing, washing dishes, etc. In a few cases bodily automatisms which took the form of an unconscious acting out of the dream were reported, while in others they were quite unconnected with the images of the dream. Mr. Lindley, in his study of the phenomena of mental effort, reached the tentative conclusion that "many automatisms represent processes for the production and maintenance of central nervous energy as well as for the production of the state of attention, and this seems to hold good for states of attention where the object is internal as well as for sensorial states.[2]

The conditions mentioned as favoring day dreaming were twilight, moonlight, solitude, soft music, sound of the waves or falling water or any monotonous sound which tends to fatigue the attention, listening to an uninteresting lecture, sermon or recitation, physical or mental fatigue watching an open fire and looking at a distant landscape. It will be noted that several of these conditions are those favorable for inducing hypnosis. In a large percentage of the cases day dreaming is

[1] J. Le Conte: Sight, New York, 1881.
[2] E. H. Lindley: Motor Phenomena of Mental Effort. *Am. Jour. of Psy.*, Vol. VII, July, 1896.

either directly associated with bodily or mental fatigue, or fatigue is suggested by the conditions mentioned. Many children name the later hours of the school session as the time for day dreaming and "bed time, before going to sleep" is a favorite hour for both children and adolescents. There are many indications that day dreaming is often the normal rest of the mind which takes refuge from monotony or fatigue of the attention by this method of relaxation. Voluntary attention is fatiguing even to adults and much more so to children whose control over the finer muscles is but partially developed. Binet[1] in his experiments on the effects of intellectual fatigue found that even for periods of work occupying less than fifteen minutes, there was relaxation of the eye muscles, and that for periods exceeding thirty minutes muscular effort, as tested by the ergograph, was diminished. Mosso[2] found that while in some instances the first effect of intellectual fatigue was to increase the energy of the muscles, it was followed by progressive enfeeblement.

Bound up with this question of fatigue is the relation of attention to day dreaming. Mosso considered in his experiments that attention was completely dispersed, when after an effort to make his mind a complete blank, images entirely uncontrolled by will trooped unbidden into his mind. But while this absolutely passive play of association is reported in a few cases, it is by no means the most typical form of day dreaming. From the teacher's point of view, the day dreaming pupil is certainly in a state of inattention, but in far the greater number of cases even voluntary attention is not wholly suspended, for the choice of subject is initially determined and if the subject of the dream becomes unpleasant it is usually changed or the dream is banished. Instead of complete dispersion of the attention, there is a withdrawal of it from the perception of outward things and a greater or less degree of concentration on the mental content. Usually this attention is of the passive sort but, even this is not always true, if in day dreaming are included those forms of story building which are worked out in logical sequence though possibly quite irrespective of their connection with facts. The tendency of day dreams to become more of the passive type is distinctly increased toward adolescence and danger of impairment of attention from over indulgence is clearly recognized. This is brought out in the opinions as to the rightness or wrongness of day dreaming which will be discussed later.

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[1] A. Binet et V. Henri: La Fatigue Intellectuelle. Paris, 1898.
[2] Fatigue; A. Mosso, translated by M. and W. B. Drummond, London, 1904.

The Content of Day Dreams at Different Ages.

In reading successively the papers furnished by the different school grades the change and increase in variety of content was very noticeable and there were a sufficient number of papers from the same grade in different localities to bring out differences fairly well. The dreams of the youngest children who could write (7 to 8 yrs.) were almost entirely of play and good times with a sprinkling of the fairy story type of dream. Memory images are very prominent and the chief imaginative alteration consists in making the dreamer's self the chief personage of the dream. The particular plays and ideas of a good time vary with the environment, as all classes of children from the rich to the extremely poor are included, but are reducible to a few fundamental interests, namely, plays which involve motor activities and out of door life, nature interests, especially in connection with animals, and eating. The plays and games of boys and girls show some divergence but out of door life and activity figure largely in both. Nature interests were especially noticeable in the returns from the Worcester schools and showed a greater variety than in returns from other localities. Images of good things to eat play quite a large part in the consciousness of both boys and girls, figuring in the fairy stories, picnics, excursions, birthday parties and Thanksgiving celebrations as well as by themselves. The eatables oftenest mentioned were candy, ice cream, cake and fruit of various kinds. In the returns from very poor children these dreams of eating were pitifully prominent and evidently affected by the physical conditions of ill nourished bodies, though the interest seems to be a perfectly normal one for children of all classes. The effect of insufficient nutrition on mental states is further brought out in some material[1] from Polish peasant children in which the questions "what is happiness" and "what is your greatest wish" were asked. "To have enough to eat," "never to be hungry," "to have enough bread," were the typical answers. For girls from eight to ten, the fairy tale form of day dream predominates over all others. It appears to be a mental device for compassing all desires, and actual experiences and possibilities are often mingled indiscriminately with the wildest impossibilities. Nearly all dream of being rich and having every desire gratified and the dream of being a princess and living in a palace "with a piano in every room" and having unlimited silk dresses and jewels may be mixed with the wish "to have enough good food every day." The *deus ex machina* in these dreams is most frequently a fairy godmother, though wishing caps, a magic

[1]Collected for the University by Madame Anna Goudzinska, of Kiev.

lamp or ring also figure. With boys of this age, the fairy story dream is less common and the form differs from that among girls. An interesting example of this occurred in a grade where the children were evidently all familiar with the story of Aladdin's lamp and the magic carpet. Nearly all of these had day dreams of flying or being transported through the air. Nearly all the girls had preserved the original forms of the stories with slight alterations, but the boys dreamed of all sorts of wonderful flying machines, sometimes mentioning the rate per hour, of trips in a balloon or by means of mechanical wings, of which they were in some cases the inventors. The desire for riches, while quite as wide-spread among boys as among girls, seems to demand a more logical explanation of its origin than that furnished by a fairy godmother. Dreams of finding money in amounts varying from fifty cents to five million dollars occur or the dream may be projected into the future and acquiring a fortune by possible or impossible means may be imagined, but however improbable the dream there is usually an attempt at logical consistency in it. Typical examples of these dreams are the following:

M., 9. Once I dreamed of finding a fifty-dollar gold piece. The first thing I bought was a bicycle and a riding suit for thirty dollars. And the other twenty dollars I gave to my mother.

F., 10. One of my day dreams was that I could live in a lovely castle. Eat good food, fruit, and vegetables. And be a fairy and have a wand. I could have a hundred houses full of $20. dollar bills. And ride in a lovely diamond flower team. Have as many dolls as I would wish. And have doll carriages dressed in silk. It would be summer all the time. I could have white silk dresses, pink, blue and bright gay colors. I could have as many boys and girls to play with me. And I could have story books.

M., 9. Once I have thought that when I am a man I should like to be a millionaire and have a house with green grass as far as I could see. And a hundred horses, fine runners. And every day go out on some lake on a canoe and have a man to take care of a canoe better than anybody else. And the best horses in the world and all the things I could think of, I could have.

F., 10. I want to be a king's wife and live in a large castle. And have a great many rooms and in each a nice piano. And have a long silk robe of red, pink and many other colors. And have a Morris-rocking-chair with diamonds and rubies.

These childish dreams of wealth rarely show traces of the commercial instinct. It is always a means rather than an end, and children in whom the commercial instinct is strong are apt to have a much less imaginative type of day dream. They dream, but their mental images are much more closely related to facts. One boy of ten dreamed of playing marbles with another boy and that "he skun all his marbles." Good trades and means of actually earning money also figure in this type of day dreaming where images are usually furnished by the

immediate environment and undergo little change. Dreams of wealth characterize the reveries of children of all ages and adolescents, but the vision of wealth ceases to be of the fairy story type and the golden palaces, gorgeous jewels and dresses of the childish dreams fade and are replaced by those of a more materialistic character. Wealth is no longer imagined as the gift of a fairy godmother but as acquired through material agencies. Boys dream of acquiring a fortune by means of some wonderful invention, by going West and discovering a gold mine or by phenomenal success in business or speculation, but whatever the method, it is always a short and easy process. Girls dream of marrying millionaires, inheriting large fortunes from newly discovered relatives or of becoming famous actresses, musicians or authoresses and acquiring wealth along with fame. Frequently there is a strongly altruistic element in these dreams, especially during the early adolescent years. Hospitals are endowed, animal refuges established, public play grounds fitted up, fresh air work carried to an extent which quite dwarfs its present proportions, the poor are clothed and fed, and one young philanthropist would "give every boy a bicycle." The part which bicycles play in the consciousness of the American boy, and sometimes of girls, also, is astonishingly large. Those who do not possess them dream of having them, and those who have them dream of the good times they have had or expect to have. A boy of eleven writes:

"I dream most often of having hundreds of dollars and I go down and order two bicycles and have coaster brakes put on them. Then I bring down my brother and get the bicycles and order bicycle shoes and suits." Another, of fourteen: "I dream most of riding a bicycle. Once I dreamed that I and some other boys were racing. We had to go around the track three times and I won the race, the other boys coming in a few yards behind, and there were thousands of people looking on."

A few years later automobiles take the place of bicycles and the desire to own one is wide-spread though not as universal as in the case of the bicycle.

The following dream so completely sums up the various sports indicated in the "good times" of day dreams that it is given entire.

M., 14½. My day dream is if I had $16,000,000 I would have a couple of red devil automobiles, a couple of air-ships, and a fine big mansion. I would have a couple of hundred nice carriage horses. I would hire a couple of hundred of men to take care of things and keep everything looking swell, and a swell big building for playing in-door-baseball in winter, Basket ball, Ping Pong, Rugby, and all kinds of sports and games. The first thing I would do before I ate my breakfast would be to go out and have a nice swim and then take a good pair of

Arabian horses and take myself out for a ride and then come back and eat a good breakfast and take one of my red-devil automobiles out for a good ride.

A more modest dream by a boy of the same age is "I would like to have a snug little cottage by the sea, and have a small yacht and few row boats and be able to go out in them whenever I please."

With the average healthy boy from ten to fifteen a large part of his day dreaming appears to be connected with sports and athletics. When tired or not interested in his school work he is apt to take refuge from *ennui* in visions of fishing, gunning, marbles, base ball, swimming, camping, boating. Even when these amusements have little likelihood of becoming realities, he still conjures up visions of what fun it would be if he could have them.

Base ball furnishes the content of many day dreams for boys from twelve to sixteen. The character of the dream varies from reviewing a recent game in all its details or anticipation of games in the near future to dreams of greatness as a famous pitcher in which the applause of admiring multitudes is vividly pictured. In some cases the dream is so vivid that incipient movements connected with the game are made.

M., 13. One day in school, I got to thinking what a fine time I would have playing ball after school. I dreamed that I was a fine pitcher in a team and the other boys were glad to let me pitch.

M., 13½. My dreams are mostly about ball games and I don't enjoy them very much because they make me think I am a whole lot and then when I wake up I'm nothing but a boy that can't play very well at all."

Dreams of hunting, fishing, swimming, being a cowboy and living on horseback and travelling in unexplored countries are characteristic throughout the teens. Some of these dreams are entirely unconnected with experience while in others the mental imagery is largely furnished by memories of vacation pleasures. The instinct itself appears to be widely spread and independent of environment, since it is apparently as strong in those who have never been outside a city as in those who have had opportunities for gratifying it. Very few girls have these dreams and they are usually in the form of wishing to be a boy so that such things were possible. Girls dream of travel as much as boys, but when details are given they are of comfortable, civilized travel and rarely include elements of adventure. Country life and animals also figure largely, but very few drift beyond the bounds of convention in their imaginings. The chief form in which any inclination toward adventure appeared was in the dream of being a red-cross nurse and going to China or the Philippines.

Dreams of fame and future greatness rarely occur before

adolescence. They vary from vague dreams of achieving honors in military or naval service, law, medicine, politics, music, acting, winning social or business success, to the attainment of some coveted school honor, having the highest works, gaining honors at graduation or being a leader in athletics. To the boy looking forward to college the highest pinnacle of fame seems often to be the attainment of captaincy of a football team. In children the desire for self recognition and aggrandizement demands immediate fulfillment and is rarely projected beyond the immediate future, while to the adolescent the vague future seems to possess special attraction and this distinction seems to hold throughout all dreams of the future. A child's vision of future pleasure is usually bounded by "next Saturday" or the nearest vacation, while the adolescent range seems to include past, present, and a boundless future. An apparent exception to this occurs in children's dreams of being grown up, which are very common, but when these are described in detail they almost all prove to be of the fairy story order and not a real looking forward into the future. The content of these adolescent dreams of future greatness are chiefly dependent upon environment and personal ambitions. Some are a mere expression of desires without expectation of fulfillment while others show evidence of being a distinct source of inspiration for purposive effort. Several writers state that when tired or discouraged they found in these dreams of future success encouragement and inspiration for further effort.

F., 13. My day dream is mostly about being an actor in an opera company. I dream of being a beautiful singer.

F., 13. Sometimes I dream of being an authoress and travel all over Europe and Asia, writing about the different peoples.

F., 15. My day dreams are of being a fine pianist and having people overwhelmed by the beauty of my playing.

M., 15. Dreams of becoming the champion ball player of the world.

M., 16. Dreams of military greatness and becoming a great general.

M., 19. I dream of future greatness. I do not think my day dreaming an evil because it places before me an ideal to strive for.

F., 18. I dream of becoming famous as a writer or actor or other impossible person.

M., 19. As I have always wanted to be a lawyer, my air castles have always been of palatial law offices, stump speeches. Congress and the inevitable White House vision looms in the background. Every boy dreams of the presidency. I see myself delivering a powerful speech before some large audience, with roars of applause interrupting. I think it a bad habit and wish I could stop it. It interferes with study and makes me dissatisfied with reality.

M., 18. Dreams of becoming a famous engineer and overcoming great difficulties in problems of construction of bridges and railroads. Never dreams except in leisure time and thinks that he works the harder because of these dreams.

From the age of twelve the influence of books upon the content of the day dream becomes increasingly important. With

the less imaginative the dream may be merely a reproduction, with slight alterations, of some book recently read, but in other cases the book simply furnishes the raw material out of which the fabric of the dream is woven. Girls put themselves in the place of their favorite heroines and adapt the material of romance, poetry or travels to their own uses. Their ideals of life are affected by what they read. Some of these dreams of the future are visions of beautiful and useful womanhood, but the trail of the Elsie books, with their morbid religiosity, and the influence of the Duchess and Rhoda Broughton is evident with unfortunate frequency. Boys dream of fighting indians, having hair breadth adventures on land and sea, being cowboys, pirates, brigands or national heroes as the case may be. Detective stories seem to acquire a peculiar charm at about the age of fourteen. The best of these do not apparently exercise any particular harmful influence and they appeal strongly to the logical instinct which seems to acquire prominence at about this age. But of the baneful effects of the worse class of this literature there is no doubt. Boys become familiar with the details of sin and crime before their moral ideas are fixed. The qualities of courage and hardihood involved in certain forms of crime appeal strongly to their imagination and many cases of juvenile crimes are directly traceable to literature of this sort. A more common effect is the lowering of ideals of manly honor and pure mindedness and the taste for emotional excitement which renders other literature tame and uninteresting and destroys interest in school work.

Another type of day dream common to both children and adolescents is the story making impulse which in some cases reveals a high type of creative imagination.[1] Some children regularly get themselves to sleep by making up stories, the same one sometimes being continued for several nights. The frequency of the continued story was, however, very small, forming in the present collection of data less than one per cent. of those mentioning the story form of dream.

F., 17. When eight or nine years old I used to sit at the window and make up stories about people who passed.

F., 18. Once when I was about thirteen years old, I was sitting in the twilight rocking the baby's cradle. I felt sad and lonely and singing softly to myself I made up a song about my husband being far across the ocean and never would he again come home to his loved ones; then it ended by his sudden home coming and joyful welcome.

F., 18. My day dreams are in the forms of imaginations in every way remote from my surroundings. They are somewhat in the form of a story whose incidents and scenes are continued from time to time. I have recorded some of my day dreams in the form of stories.

[1] The Continued Story, Mabel W. Learoyd. *Am. Jour. of Psy.*, Vol. VII, p. 86, 1895.

F., 18. My day dreams frequently deal with some adventure in which I am taking an active part. They are like stories and unfold themselves gradually. Since childhood I have been in the habit of putting myself to sleep with these dreams.

F., 19. My day dreams have always been stories. I used to get my little brother to sleep by telling my dreams. I still dream stories, and sometimes I dream of becoming famous.

F., 30. I have since a small child put myself to sleep nightly by spinning stories of adventure, romance, and travel in which I was the chief actor.

Closely akin to the story form of day dream is the imaginary conversation which is sometimes carried on with actual friends and acquaintances, sometimes with strangers casually seen or with characters in books or history, or in some cases with purely imaginary characters. Some novelists and dramatists have done much of their composing in this form, and these imaginary characters acquire a vivid personality. Lonely children sometimes develop this form of imaginary companionship. Some years ago Dr. G. Stanley Hall collected a number of cases of these imaginary companions and the records are given with considerable detail. All began at an early age, usually as soon as the child began to talk, and continued for several years, usually until the child began to go to school, or was otherwise brought into contact with children of the same age. One child, a boy, began to play with an imaginary "Gobby" as soon as he could talk and when nearly five "Gobby" was still his constant companion, but had grown up and had a wife and daughter, who were also playmates. Another child of about the same age had two imaginary playmates, one of whom was responsible for all his bad behavior, and the other played the part of his good genius. His probable behavior could often be inferred by noticing which of his imaginary companions was in evidence at the time. A few years ago in one of our popular magazines, there appeared some letters purporting to be written by a child of eleven to her husband whom she assumed to be somewhere in the world though she did not know him.[1] Whether these letters are, as they purport to be, the genuine productions of a child, or later reminiscences put into this form for literary effect, they picture with psychological truth the impulse of a lonely and imaginative child to find in an ideal world the sympathy and companionship which was lacking in the outward life.

Day Dreams of Love and Marriage are frequent after the age of seventeen, and occasionally earlier than this for girls. Sometimes these are vague dreams of a happy future with a shadowy partner who is to possess all virtues; sometimes there is a defi-

[1] A Child's Letters to Her Husband; Helen Watterson Moody. Maclures, Vol. XIV, p. 55.

nite picture of a future home and a house is planned and furnished in all details. With girls, unless they are definitely looking forward to marriage, the house planning and furnishing is usually of a luxurious character and without reference to probability. With boys this vision of a home is more apt to be controlled by the possibilities of achievement. Both boys and girls frankly acknowledge dreaming over their friends of the opposite sex, though the more elaborated romances are nearly always woven about comparative strangers or wholly from the stuff of which dreams are made. Many girls imagine themselves in a home with children to whom they give names and even picture the color of their eyes and hair, how they shall be dressed and educated, and the good times they shall have, while the shadowy partner of these joys is rarely visualized or very definitely characterized. A few samples of this type of day dream are given.

F., 18. I dream of being married and having a beautiful home of my own. I picture to myself the arrangement of the rooms. And the prettiest room in it will be a nursery furnished in pink and white and occupied by a curly headed little boy and girl who will be the dearest children in the world.

F., 17. My day dreams are usually about my future life: if I were married and had a home of my own and how cosey I would keep it.

F., 18. Dreams of certain friend, recalls all he has said in conversation, plans their future together, etc.

F., 19. My day dreams are mostly of a sentimental character.

F., 22. My day dreams are apt to be about some whom I have met or to whom I am greatly attracted. Sometimes I dream of being married and having a home. Sometimes I write imaginary letters.

F., 17. Sometimes I dream of meeting my future husband, falling in love with him, etc., and how I would love and care for my children.

F., 20. I do not have much time for dreaming now. I used to imagine the pleasure of having a little home in the country with mother. I know that I ought not to worry, so try to keep those thoughts out of my mind.

F., 19. I seldom have time to build castles in Spain but when I do I am not different from most southern girls, *i. e.*, my dreams are usually about a pretty fair specimen of a six foot three inch biped.

F., 20. I nearly always dream of myself as being very famous or at least holding an honored position. I have dreamed of being a teacher, a trained nurse, the head of some great medical institution, or a great speaker. I never dream of being a wife and mother. I cannot say why, but perhaps because I am not a pretty girl but decidedly homely.

F., 18. There are times occasionally when I think how nice it would be to be married and have a home of my own, and I think of the joy it would be to train up a little child and know that he was your own.

M., 17. My day dreams are sometimes of having a home, a loving wife and children and the means to keep them in comfort.

M., 23. As for day dreams I rarely indulge in them, don't have time. I fill my spare moments with thinking of my sweetheart.

M., 25. My day dreams are usually of her whom I hope to make my wife. I have visions of our future home life together and they are an inspiration to better living and more earnest effort.

M., 21. My day dreams are generally of what I am going to do in the future (of course a certain pretty girl plays an important part).

M., 25. My day dreams are generally made up of plans by means of which I hope to make my sweatheart my happy wife. They are not mere love dreams but contain all the essential elements that go to make professional life a success. My dreams are of reaching the highest point in my profession and making my wife happy.

M., 27. My chief day dream is of a beautiful home and an angelic wife.

The house planning form of day dream is of frequent occurrence even when not connected with dreams of love or marriage. There are many of both sexes who seem to have a sort of architectural instinct and find recreation in planning not only houses but grounds and even extend their fancies to landscape gardening and poultry raising.

A more prosaic form of dreaming is that in which the future occupation as a means of livelihood is the content of the dream. With the children of the poor, this is influenced by probability at an early age and the natural instincts for activity and out of door life find little play. Both boys and girls of eight or nine look forward to earning money as soon as the legal school years are completed. Those who are able to remain longer in school look forward to nearly every possible range of occupation. The number of occupations mentioned by boys is naturally greater than by girls, but the latter mention nearly every occupation open to women, including teaching, nursing, stenography, bookkeeping, dressmaking, millinery, work in a store, etc. In most cases there were pleasurable anticipations connected with these images of the future either because of expected enjoyment in the work itself or because of the prospect of earning money, often with desire to benefit others.

Opinions as to the Rightness or Wrongness of Day Dreaming.

The youngest children who wrote their day dreams (those of the third grade ranging in age from seven to nine) had evidently not thought of a moral aspect of day dreaming and either gave no answer to the question or expressed surprise at its being asked. One child answered: "No one ever told me it was wrong;" and two or three others thought that it was right if the things dreamed about were true but wrong if they were not. The papers of the children giving this answer showed rather a high degree of imaginative power and it would be interesting to know whether they ever told their dreams as facts. Several reminiscent papers mention the confusion of fact and fancy in childhood, and one girl of eighteen states that at the age of fourteen her day dreams were so vivid that she sometimes told them as facts. In all grades higher than the third,

day dreaming and inattention to lessons seem to have become inseparably associated and the answer is apt to be of the stereotyped form that day dreaming is wrong in school "because you ought to be attending to your lessons." But in addition to this reply many children appear to have done some independent thinking and give individual reasons for thinking the indulgence right or wrong. Day dreams are wrong if they are about bad or mean things, wrong "because they make you feel cross when you are interrupted," "because they make you dissatisfied with what you really have," "because it is wrong to wish for what you can't have," "because they waste time." They are *not* wrong "because they are natural," "because they can't be helped," "because they are about pleasant things." They are right because they make you happy, make you forget your troubles and worries and because "they rest the mind." Typical specimens of the answers given at different ages are given.

M., 11. I do not think it is right to let your minds wander off. Sometimes my mind will wander off. I will not know what I am doing. When I try to think, it is very hard to think. Sometimes it will be a long time before I can think what I am doing.

F.. 13. Day dreams are wrong because they make you feel cross when you are aroused.

M., 11. "I think children should try to stop themselves from having day dreams because when you are dreaming like that in school you might miss a whole lot of lessons."

F., 12. Dreams oftenest of her brother who is dead. "My dream is wrong because my brother is dead and of course could not speak to me."

M., 12. I think it is one wrong thing nature lets us do because you might want to do something very bad and sit down and dream away your time.

M., 14. I do not think day dreams are right because the more you let your mind wander the more you want to be there and know you cant.

F., 15. I think day dreaming is wrong because I have not very much thinking power and I think they use up a good deal of it.

M., 15. Last year I would sit in school and think of everything but my lessons. I failed on the final examination.

M., 13. I think these dreams are wrong and one ought to guard against them for when you get bigger they will happen more often and then people will call you dreamy head.

F., 18. "I think they are wrong because they weaken the mind."

F., 20. I think those that I do not enjoy are wrong, since in them I often attribute less generosity than I should to those with whom they are connected.

F., 12½. I think too much day dreaming is not good for anybody but when there is nothing else to think about they are very good things to have for they keep the mind off dwelling on troubles.

F., 13. I do not think my dreams are wrong for I hardly ever think of anything wrong.

M., 13. I think them right because it don't hurt you any to think, but I think it does in another way, this is in letting your mind go where it wants to, not taking care of its own business.

F., 14. I don't think them wrong if they aint about things which could make anyone do wrong.

M., 14½. I think they are right unless you ought to be doing something else because then you are not thinking of tricks to do and they keep you out of mischief.

F., 12½. They always seem right to me because nothing happens like the things I dream.

M., 13. My dreams are always right because they are true and always happen or are happening when I dream.

F., 15½. I think these dreams are all right because they do not hurt anyone. They are just childish thoughts.

M., 12. I think they are more right than wrong because when you are sad you could sit down and dream and forget your sadness.

F., 14. They give your mind a rest a great many times, but sometimes they take your mind off what you ought to be doing.

F., 17. I do not think day dreaming wrong unless carried to excess, and furthermore I think it encourages me.

M., 18. I do not think them wrong when I have leisure for them. When tired I like to let my mind drift away because I think it refreshes me and stops all the worries I may have.

F., 18. I enjoy day dreaming very much but I sometimes think it is wrong for it is apt to make you dissatisfied with your present life.

F., 18. I do not think it wrong to day dream. It gives one something to have as an ideal object in life. He plans his life in many instances according to his day dreams. Even if he is never able to carry out all his dreams, they give him a great deal of pleasure.

F., 19. Day dreams are often an inspiration to higher things. They sometimes lead us on to try to reach our ideals.

M., 19. This day dreaming seemed to force itself upon me. I tried hard to resist it because I thought it injurious to my mind. The more I day dream the harder it is to come back to reality.

Only a small per cent. of children above the fifth grade (10 to 12 years) and adolescents say that day dreaming is right without qualifying the answer in some way. "It is not wrong unless" or "right when it does not interfere," etc. Many adolescents give an unqualified "wrong" in answer to the question basing their answer upon personal experience. Some state definitely that their power of attention has become so impaired that any work requiring effort or continuity of attention is difficult and irksome. Others, taking a broader view of the subject, consider that while excess is harmful, a moderate indulgence under proper conditions of time and place is restful to the mind and, in some cases, is an inspiration which tends to widen the mental horizon. The insidious tendency of day dreaming to usurp the place of other mental activities, is, however, very generally recognized by adolescents and adults, and those who most fully recognize its value as a normal rest and relaxation of the mind or the soil from which real creative work may spring, appreciate as well the danger that the servant may become the master and mental imagery control the mind even in opposition to an effort of will.

Relation of Mental Images to Day Dreaming.

[1]Galton was the first to call attention to the great difference in character of the mental images in different individuals, his investigations showing that while some persons can call up mental pictures which are distinct and vivid in color and outline, others are so deficient in this power that the term mental image appears to them a mere figure of speech. Galton found that philosophers and those accustomed to abstract thinking were apt to be deficient in this power while children were likely to possess it in a high degree. He also found the visualizing power to be somewhat higher in the female sex. Binet,[2] following to some extent Galton's method, has gone somewhat farther and made a study of the degree to which mental images are under the control of the will. In addition to his more general investigations, Binet had two subjects (sisters) whom he studied with great care through a series of years and in whom there was a marked difference of type. Both were able to call up visual images though one did so with greater effort, her images were less complete than those of the other and she had little power to alter or transform them. When, however, the images were allowed to arise spontaneously, as in the more passive forms of day dreaming, there was great variety and richness of imagery. The younger had exceedingly distinct memory images and possessed the power of voluntary control over them in a high degree, altering them quickly and easily in accordance with suggestions made by the experimenter. Her mental imagery was, however, almost entirely lacking in spontaneity, and she seemed unable to comprehend that these images could arise apart from an act of will. Both of these types were abundantly illustrated in the present material. Many children described the succession of mental images which passed through their minds, said that they came of themselves and could n't be helped. Some described day dreaming as "queer" or "funny" because you never could tell what was coming next. Others described their day dreams as a definite reproduction of scenes which they had especially enjoyed, or said that they had some favorite day dream which was voluntarily initiated. Still a third form of day dreaming in the broader sense is illustrated by the insistent imagery which appears even in opposition to the will, as in the case of a boy who said that he did n't enjoy day dreaming because the one which came oftenest was the repetition, with all its details, of an accident in which he had seen his uncle injured. Dramatic authors sometimes have trouble with

[1]Francis Galton: Inquiries into the Human Faculty, London, 1883.
[2]L'Étude Experimentale de l'Intelligence; A. Binet, Paris, 1903.

their characters who persist in behaving in a way quite in opposition to the ideas of their creator. It is probable that these insistent ideas, which are usually connected either with some emotional shock or strain or are an accompaniment of overfatigued states, approach very near to the line of morbidity. The relation of the will to day dreaming, as seen from the children's point of view, is of interest in this connection. Some say that day dreaming is not wrong because "you can't help it and what you can't help can't be wrong." Others say that they "can't help it sometimes" especially if tired or not interested. One boy says that he can't help it in school but is never troubled that way when the subject is base ball. Many state that they voluntarily initiate day dreams as a means of passing the time when lonely or uninterested or as a refuge from unpleasant actualities.

Relation of Day Dreaming to the Creative Imagination. There are a few adults who say that they never day dream but their papers show that they have restricted the meaning of the word to an exercise of the imagination which has no foundation in fact and which has been set aside as a childish mode of mental action. In the broader sense of the term it is probable that every normal mind exhibits certain automatisms in its reproductive activities, whether these be unaltered memory images or imaginative transformations and combinations which are a true creative activity. The richer the content of the mind, the greater the variety and spontaneity of the day dream and the greater the possibility that from its automatic working new and original combinations may arise. A psychological study of inventors would probably reveal the fact that many of the great inventions, though sought and worked over for years have come, at last, in a flash of insight through the automatic working of a mind filled with all the possibilities of the subject. Indeed we know this to have been the case with many scientific discoveries; and the biographies of artists, authors and scientists emphasize the fact that many of them have been day dreamers in boyhood, but always along with this has coexisted the fact of special interest and activity along some particular line, even though there were deficiencies in other directions. Herbert Spencer has recorded in his autobiography the fact he was, during his boyhood, "extremely prone to castle building" and that the habit continued even into mature life. This habit, while usually indulged in at bedtime, was frequently a course of annoying absent mindedness. In later years he wrote: "I believe that it is a general belief that castle building is detrimental; but I am by no means sure that this is so. In moderation I regard it as beneficial. It is a play of the constructive imagination, and without constructive imagination

there can be no high achievement. I believe that the love I then had for it arose from the spontaneous activity of powers which in future life became instrumental to higher things."[1] Many facts from the biographies of the world's leaders can be adduced in support of this opinion of Spencer's, and it may well be questioned whether too vigorous a pruning and repression of this play of the imagination is good pedagogy and whether a certain amount of this mental recreation is not necessary for mental growth. We know that music, art, and literature are much indebted to the great dreamers. But the mind must first be well stored and there must be energy for the realization of the dreams. It is never to the idle dreamer that the creative impulse comes. Mozart and Raphael were dreamers, but the harmonies of the one and the visions of the other belong to the world only because their dreams received embodiment by alliance with the drudgery of practical work. Napoleon and Mohammed were, each in his own way, dreamers, but they were also men of action. To Gautama, only after years of mental striving came the perfect rest and the vision of Nirvana. It is probable that to most artists the vision beautiful comes when the mind is passive and visual images rise unbidden, and literature owes much to that spontaneous play of imagery which is one of the characteristic forms of day dreaming. We do not need to recall that strange fragment of Coleridge's dream, Kubla Khan to realize that the brains of poets have sometimes worked in an automatic way. The day dream shades by almost imperceptible gradations through hypnogogic states to the dream of sleep, and, as those whose mental content is fullest are those who are apt to dream most, so with the day dream. Babies and idiots probably do not day dream as they have not a sufficient store of mental impressions for reproductive combinations. And among those whose lives are a monotonous round of toil in the bare struggle for existence, there are probably few dreams either of the day or night, because little material is furnished by the environment. Experience having bred few images for the fancy to work upon, release from bodily exertion is followed almost immediately by sleep. The effect of monotonous labor in dulling mental images, even in well stored mind, is noted by those who have spent years in Siberian prisons, even the images of home and friends being no longer recalled with clearness.[2]

Enjoyment of day dreaming in itself considered, except in those cases which are either morbid or tend to become so, is universal. The few who say that they do not enjoy it invari-

[1] An Autobiography; Herbert Spencer. New York, 1904.
[2] Sixteen years in Siberia; Leo Deutsch. London, 1904.

ably give as a reason, conscientious scruples in regard to it as the factor which disturbs enjoyment. Children occasionally give some unpleasant consequence resulting from the habit as a reason for non-enjoyment but nevertheless do not discontinue the habit. Some say that day dreaming is their greatest pleasure and that they "could not live without it." Even sad dreams are enjoyed, the sadness being of the same nature as that evoked by seeing a tragedy on the stage or reading a book which may be thoroughly pleasurable even though the reader is reduced to tears. Emotions in day dreams of a normal type are all attuned to a low key, due, perhaps, to the relaxation of the muscular and vascular systems. Mosso[1] found experimentally that respiration tends to become periodic, and the pulse is lowered when attention begins to wander in states of drowsiness and in the dreamy states when attention is most completely dispersed. Twilight moods of reverie are typically characterized by the more subdued emotions and by moral and religious aspirations.[2] The mood is generally enjoyed and many say that it rests and helps them.

Morbid Day Dreaming.

In cases of morbid grief and painful reverie instead of muscular relaxation there is sometimes a partial paralysis and rigidity of the muscles which is apparent in the face and hands, and in the character of the movements when the subject is aroused. These cases of painful reverie are reported chiefly by adults and are sharply distinguished from the enjoyable melancholy and "sweet sadness" of normal reveries. The content is not an imaginary situation, but some actual sorrow or trouble, and the tendency to morbidity is frequently recognized by the subject, and is shunned by an effort to keep the mind occupied with other things. In cases of physical weakness and ill health, these reveries tend toward, and in some cases become obsessive ideas against which the patient struggles in vain, whenever physical weakness prevents constant occupation. Scenes which crush the heart and paralyze effort are relived again and again and the will is powerless to banish these images which the patient may fully realize are leading to mental degeneration. In the entire number of day dreams collected from children only thirteen morbid cases occurred as regards content, though there were a number of cases in which, though there was no morbidity of content, day dreaming had become

[1] Fatigue; A. Mosso, translated by M. and W. B. Drummond. London, 1904.

[2] Reactions to Light and Darkness; G. Stanley Hall and Theodate L. Smith, *Am. Jour. of Psy.*, Jan., 1903.

so excessive and so imperative a habit as to be regarded as a morbid development, very closely approaching the effects upon some hypnotic subjects in the loss of will power. Among the cases of morbid content two were of snakes. Both subjects were boys and in one case the cause was stated as due to a fright which had generated a morbid fear which resulted in images of the object dreaded whenever the mind was allowed to wander uncontrolled. In the other case, no information was given beyond the fact that day dreams were always of snakes and not enjoyed. Two children of thirteen and fourteen dreamed of dying and of the end of the world, and in one of these cases the tendency of the dream to become an imperative idea was marked. Two others habitually have sad day dreams and in both of these cases the health was reported below normal.

F., 12½. When they are nice and not frighting I enjoy them, but when they are horrible and frighting I do not like them.

F., 13. My dreams are most often different but about something sad.

M., 14. Day dreams chiefly of snakes of which he is afraid.

F., 13. When I am sewing or reading, I begin to think. I think and think about everything until I think about something I cannot get off my mind. One thing I dream about most is the end of the world. I wonder what will become of the people and how the earth will look and how dreadful it will be.

F., 14. I always think about the past and what if I should die.

F., 19. I am a victim of day dreams to a most annoying degree insomuch that all efforts at resistance seem futile.

F., 39. Has met with great loss and sorrow; sits for hours in the same rigid attitude, with eyes fixed on vacancy. When aroused makes an effort to attend to things about her but if left alone sinks back into the same attitude. The images of her sorrow are constantly before her mind.

Sir James Crichton Browne[1] inclines to the view that all dreamy mental states have a morbid tendency. He acknowledges that in otherwise healthy minds no harmful consequences either mental or physical can be detected. He quotes various cases in connection with nervous and mental diseases, such as the dreamy state which sometimes forms a distinct aura in epilepsy and argues that men of genius known to have been subject to these dreamy states have suffered injury and been hampered in their work by them. As an extreme example he quotes the case of John Addington Symonds, the historian of the Renaissance, who suffered from a peculiar dreamy state which he thus describes. "Suddenly in church or in company, when I was reading and always I think when my muscles were at rest, I felt the approach of the mood. Irresistibly it took possession of my mind and will and lasted what seemed an eternity and disappeared in a series of rapid sensations which

[1] Dreamy Mental States. London Lancet, July 13, 1895.

resembled the waking from an anæsthetic influence. One reason I disliked this state was because I could not describe it to myself. It consisted in a gradual but swiftly progressing obliteration of space, time, sensation and the multitudinous factors of experience which seem to qualify what we are pleased to call ourself. At last nothing remained but a pure absolute self. The universe became without form and void of content." This description is very closely analogous to those states sometimes experienced in extreme fatigue when, for an instant, the mind seems to stop working and then goes on. It differs from unconsciousness in the fact that the blank is felt though no effort of memory can recall any mental content. Such states are merely results and symptoms of extreme fatigue, and unless the fatigue be sufficiently prolonged so that the nervous system loses its normal recuperative power, have apparently no more serious consequences than any other fatigue states. As in the 1,080 cases furnished by the graded schools all the children present wrote and with the few doubtful exceptions already noted day dreaming was reported by all the children with a very small percentage of morbid tendencies, there seems to be no ground for the assumption of any morbid connection, either mental or physical, with day dreaming *per se* more than with any other mental activity. If morbid cases are sought they are not difficult to find either in the form of morbidity of content or excessive indulgence, resulting in loss of will power, or cases in which both factors are combined. Féré[1] cites an interesting case of a man who had been from childhood an inveterate day dreamer to an extent which seriously affected his college course. He had pursued in his dreams a number of fictitious careers, military, marine, engineering, etc., which he seemed to prefer to real life. On leaving college, however, he engaged in an active business career, was happily married, successful in his undertakings, and having no time for day dreaming, seemed to have overcome the habit. A few years later, however, he began to suffer from insomnia and at the same time became dissatisfied in regard to his business and household affairs. He took refuge in his former imaginations, and though these were less absorbing than formerly they gradually became more persistent and finally acquired a fixed form in which he lived an ideal life in a chateau which he gradually elaborated. He acquired an imaginary wife and children and manifested less and less interest in his actual family. He continued nominally to conduct his business, which, however, was really managed by his staff of employees. Finally, on an occasion when some one accosted him by name and wished to

[1] Pathology of the Emotions; Ch. Féré. London, 1899.

confer with him on business he replied "He is at Chaville," the name of his imaginary chateau. This betrayal of himself in public, however, startled him into a realization of his actual condition, and fearing himself insane he was ready to do anything to banish his ideas, but found that they had become his masters and that against his will he constantly relapsed into his dreams. After three months of medical treatment, with strict supervision night and day to prevent any lapse into dreaming, he recovered. In this case visual images appear to have played an important rôle and the subject was of a strongly visual type. Whether in this case, the day dreaming was the cause or result of a diseased mental state is uncertain, but the suppression of the dreams was an important factor in the treatment which resulted in his recovery. As to the danger of day dreaming in a normal individual the following testimony of a man of twenty-six, who has carefully analyzed his own case, is of value. A. B. remembers that as early as the age of eight years he was a dreamer and says that his day dreaming has been the happiest part of his life but that "it has made it very hard, sometimes next to impossible, to pay attention to anything dull or abstract." All the will power I can bring to bear only serves to pull my mind back to what it ought to be busy with instead of keeping it steadily focused there. If one could dream up to the limit when one ought to dismiss it entirely and attend to the sterner things of life, I think day dreaming would be a veritable gift from the gods. But it is a curse when the habit becomes so fixed that a man can't pay attention to things which perchance have little natural interest for him."

The tendency of day dreaming to become habitual and excessive is, in the present study, most marked in those who have strong visual imaginations, yet the power is in itself a mental gift even though it sometimes prove a dangerous one. The great literary and religious dreamers have usually been men whose visual imagery was exceedingly vivid. Dante, Milton, Mohammed and Swedenborg were all endowed with all the power of visual imagination to an extraordinary degree. Many drugs owe their peculiar fascination to their power of intensifying sensory images and producing dreamy states. The Mexican drug, mescal, the use of which as a religious cult among the Southern Indian tribes of the United States has spread in spite of efforts to restrain it, has for its chief mental effect the production of colors and forms of wonderful variety and intensity. The muscular relaxation noted as characteristic of day dreaming is produced by all anæsthetics and where the oncoming of unconsciousness is not too sudden the mental states preceding are closely analogous to those of day dreaming. De Quincy, who more vividly than any other writer has

depicted the effects of opium, emphasizes the impairment of muscular power and corresponding weakness of will. The effect of nicotine in producing dreamy mental states is too well known to need description, and teachers report that boys who are addicted to cigarette smoking are invariably dreamers and defective in the power of voluntary attention.

In summarizing the results of the present study, attention is drawn to the following points:

Day dreaming appears to be a normal and well nigh universal phenomenon in children and adolescents and may continue throughout life. It is especially characteristic of the years of adolescence.

The content of the day dream is chiefly determined by environment, though its forms, like those of night dreams, are influenced by age, health and degree of mental development.

In early childhood, day dreams, except in the case of exceptionally imaginative children, are made up chiefly of memory images, actual experiences or stories being reproduced with little change. This tendency to reproduce memory images unchanged is evidenced not only by the day dreams reported, but is further illustrated by the insistency of children that stories told to them shall be repeated without any change in the details, a fact familiar to every one who has had experience in telling stories to children. The future of childhood is usually a definitely circumscribed and near future, and motor activities and eating figure largely in the content of childish dreams.

With the dawn of adolescence there is a marked increase in the variety and complexity of content and the range is greatly widened. Dreams of the future are oftenest of the vague future with boundless possibilities. The instinct emotions become an evident factor and dreams of love are characteristic at this age. Both altruistic and egoistic emotions are greatly intensified.

Though comparatively few day dreams were collected from adults, the content of these indicated a somewhat closer connection with actual life than those of childhood and adolescence. Dreams of the future were more in the form of plans with the possibility of accomplishment either for self or others.

The few cases of the day dreams of old age were almost entirely memories of the remote past and much time was spent in dreaming. Since day dreaming is closely associated with fatigue states this appears to be the result which might be expected from mental and physiological conditions.

Though environment exercises an important influence upon the development of the imagination and there is a possibility that it may be dwarfed and starved by repression, much is due to differences of mental endowment, and day dreaming in a

marked degree is often associated with high intellectual endowments and creative ability.

Day dreaming, like any other mental activity, may become excessive and pass over into pathological states, and in consequence of the fact that it is usually enjoyable and a passive state, it is peculiarly liable to this source of danger.

Sex differences are especially marked in day dreams, many of them being so characteristically masculine or feminine that the sex of the writer is unmistakable. While this is in part undoubtedly due to environment and conventional training, it also suggests that in the more automatic workings of the mind there may be a fruitful field for the investigation of the question of how far mental differences between men and women are innate and fundamental and how far they are due to artificial causes.

REACTION-TIMES AS A TEST OF MENTAL ABILITY.[1]

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This paper is an attempt to show (1) that a distinction must be made between what may be called the 'laboratory' and the 'anthropometric' types of reaction experiment, (2) that many of the reaction-tests conducted upon school-children have not conformed to the usual requirements of the 'laboratory' method, and (3) that reaction-time tests, of whatever type, cannot be successfully used in tests of school-children, and fail to indicate mental ability. I shall not try in what follows to keep these propositions sharply distinct, or to keep to the order just given.

The interest in reaction-times as studied in the earlier investigations (incited by the phenomena of the 'personal equation' and Wundt's complication experiments during the early sixties) was directed very largely to the determination of the time-values themselves. But it was soon discovered that the results of different investigators failed to evince conspicuous harmony, even when the outward conditions seemed entirely identical. Thus attention was turned to the qualitative analysis of the psychical or psychophysical conditions governing the process of reaction. The treatment became qualitative rather than quantitative. Naturally many factors were unearthed,—some of them but little suspected in the previous work. It was found, for instance, that the simple reaction-time depended not only upon such objective factors as the modality of the stimulus, its extent and intensity, upon the nature of the movement of reaction, etc., but also quite as much upon certain more subjective conditions, such as the degree of fore-knowledge or expectant attention aroused in the observer, the presence or absence of a warning-signal, the length and uniformity of the interval between this signal and the stimulus, the presence or absence of distraction, the general condition of the nervous system as affected by drugs, fatigue products, etc., and, perhaps most significant of all, upon the direction of the observer's attention,—an influence cited by Orschansky in 1887, but still better known by the work of Lange soon after published.[2]

[1] A paper read at the Meeting of Experimental Psychologists at Ithaca, in April, 1904.

[2] *Philos. Studien*, IV, 1888, 479.

If we recognize the fundamental character of this distinction, we must always keep in mind the possibility of attention directed toward the reception of the stimulus (sensorial or complete reaction), toward the execution of the movement (shortened, abbreviated, motor, or muscular reaction), or more or less evenly poised between these two attitudes (central or natural reaction).

It has also been intimated, and is apparently taken for granted by many investigators, that the length of the reaction-time is further conditioned by age, by sex, and, finally by another factor, possibly further reducible, that of constant individual differences.[1] It is this last factor which deserves our attention because it has been assumed by more than one investigator that this factor is either identical with, or at least a function of, general mental ability or general intelligence, and it has, therefore, been proposed to measure the mental ability of school-children in part by determining their reaction-time. Are such constant individual differences to be established by careful experimentation, and, if so, do they give us an indication of the general mental ability of the observers who exhibit them?

In answering these questions it seems to me that attention must be called to the methods of using the reaction-test in the psychological laboratory and in the schoolroom. The typical reaction-experiment, as conducted in the modern laboratory, is a qualitative experiment: the emphasis, I mean, is placed upon the introspective examination of the observer's consciousness rather than upon the quantitative examination of the time-values themselves. The experiment calls for the synthetic construction of the action-consciousness. The chronoscope is employed partly to supply the objective conditions of the experiment (stimulus, movement of reaction, etc.), partly to keep some tag upon the work of the observer,—in a word, to standardize the conditions under which the introspection is made. If, for example, the times recorded for a series of simple auditory reactions are 180, 170, 190, 450, and 60 sigma, it goes without saying that the observer is inaccurate in his introspective examination if he reports a uniform series of conscious processes in all five tests: something else than the simple impulsive action consciousness must have been present when 450 sigma was recorded; something must have dropped out when 60 sigma was recorded.

Again, it is customary in the conduct of the laboratory re-

[1] The very term 'personal equation' has this implication, and may possibly have suggested the use of the reaction experiment in the way we are discussing.

action-experiment (at least in the Cornell laboratory) to give heed to the so-called 'types' of Lange. An endeavor is made, first of all, to establish the 'natural' type by avoiding, in the instructions to the student, any reference to the direction of the attention either toward stimulus or movement, and especially to avoid the suggestion that the reaction is to be made as quickly as possible. Now, experience shows that, if these conditions are established, many observers will, in their first reactions, adopt the 'central' reaction: this is, then, literally, their 'natural' attitude toward the experiment.

On the other hand, rather infrequently one encounters a student who belongs very clearly either to what has been termed the 'subjective' or to the 'objective, type of observer.[1]

We have here, then, a definite indication of constant individual differences, but it is not an indication from which we can argue at all to fundamental differences in general mental ability. We find merely that these occasional observers, who are extreme in their type, fall naturally into the sensorial or into the muscular type of reactors in the absence of more definite instructions.[2]

Obviously when these observers pass to the sensorial and muscular types of reaction and are instructed specifically how to direct the attention, they find the one attitude easy and the other difficult, according as their type is objective or subjective.

But a question of further significance now arises:—can those possessed of a strong *Anlage* (whether this is regarded as a

[1] This is a distinction which has received an increasing amount of sanction in the recent experimental literature, and which has been well described by Stern in his treatise upon individual differences. It must be reducible to a fundamental difference in nervous constitution, and has, therefore, appealed to me as being possibly identical with Ribot's attractive scheme of the temperaments in which the fundamental division is into the groups,—sensitives, actives and apathetics. The sensitives may be the subjectives, the actives the objectives.

[2] To quote from Stern: "One man is accustomed to take up an active attitude to everything that occurs to him: his own action is for him the constant point of interest, and his environment is of importance only in so far as it affects this centre of reference; he therefore keeps his personality, his 'I,' in instant readiness for action. Another is wont to submit himself passively to the operation of external impressions; he regards them from the theoretical point of view, is contemplative in disposition. The former is inclined to make himself 'ready for the leap;' his fingers are intense, and the psyche is intent; he merely awaits the signal for action. The latter tends as naturally to a sensorial direction of attention: if we force him to think of himself, and to dispose himself for the coming movement before the occasion for its performance has arisen, he feels confined and confused. The former is expecting his own outbreak, the latter is awaiting the impression; the stimulus is in the one case the release, in the other the cause of movement." *Ueber Psych. d. individuellen Differenzen*, 108, as quoted by Titchener, *Exper. Psych.*, Vol. I, pt. II, 215-6.

matter of ideational type or of type of attention) be taught or coached to exhibit both the sensorial and the muscular type of reaction? I am aware that there is evidence in the negative. Professor Baldwin has published results from which he argues that observers possessed of strong ideational types give reactions consistent with those types despite all attempts at modification, and Messrs. Hill and Watanabe, from investigations in the Cornell laboratory, conclude that "if the volitional temperament is unfavorable, practice will have no effect in determining the two types of reaction-time."[1] Despite these statements, I have personally the conviction that, at least in the vast majority of instances, a constitutional tendency toward a particular reaction-attitude can be overcome by proper coaching. In any event, we have no evidence in these laboratory investigations that there exist any constant individual differences in reaction-time that depend upon general mental ability.

With the possible exception, therefore, of a few isolated and extreme cases, we may, in the conduct of the reaction-experiment in the laboratory, bring all observers to exhibit the two chief types of attention to the reaction. And now most important is this:—when practice is attained, as indicated by a mean variation less than 10% of the average or median value, the results, judging from my experiences in the laboratory at least, are practically identical for all observers. In other words, our constant individual differences have disappeared, since they themselves depended upon degree of practice and upon variation in the direction of attention. And so we may predict with some confidence that a practiced observer, under laboratory conditions, will give the following simple reaction-times:

	SENSORIAL	MUSCULAR
pressure	210	110
sound	230	120
sight	270	180

Those few observers who cannot be taught to exhibit both types will at least conform to these figures for the type to which they belong.

Turning now to tests upon school children and students, I wish to take for illustration the work reported by Bagley, Gilbert and Wissler. It needs but little argument to show that the results of any psychophysical test are largely influenced by the conditions under which that test is administered. The

[1] For further references, see Titchener, *Exper. Psych.*, I, Part II, 225.

conditions under which the reaction-time test has been used to determine the mental ability of school children are so unlike those just described for the psychological laboratory that I venture to term them 'anthropometric' as opposed to 'laboratory' reactions. Certainly the work of the first two investigators just named belongs in the former category. For we find that emphasis is placed upon the quantitative measurement of the time to the utter neglect of the introspection of the action-consciousness, and with the avowed intent of measuring the capacity of the subject as one might measure his skull or his physical endurance. In such tests the observers are, of course, entirely unpracticed, and no specific directions are given for the control of the attention. Moreover, probably in most cases, the pupil is given to understand, either explicitly or implicitly, that the reaction is to be made *as fast as possible*. These variations in the conduct of the experiment really *make another experiment of it*, and the results cannot be expected to coincide with those obtained in the laboratory. Hence the question must always be asked: how far are such tests reaction-times in the strict sense of the term, and, further, just what is actually being measured.

Bagley[1] attempted to relate mental and motor ability by the use of both experimental and non-experimental values. The experimental data for mental ability were the results of reaction-tests made with the aid of Jastrow's card-sorting apparatus.[2] These reactions are not, of course, simple reactions, but the method lends itself, nevertheless, to discussion in this place.

It will be noted that Bagley assumed definitely at the outset that these reaction-times would be expected to indicate mental ability. He says (p. 194): "the experimental sources of the data on mental efficiency consisted of various types of reaction-times as representing quantitatively the mental ability of the subject, mental excellence being represented by the alertness of the mind in reacting appropriately to given stimuli."

Glancing at his results, we find, however, that, by the method of correlation by groups, there is a discrepancy between reaction-times and class-standing, *i. e.*, the relation is not positively inverse, but indifferent. There follows, moreover, this significant clause: "the children whose reaction-times are nearest the norm have the best class-standing, while those who are particularly alert and those who are particularly slow in

[1]On the Correlation of Mental and Motor Ability in School Children, this *Journal*, XII, 193.

[2]It may be pointed out parenthetically that card sorting is open to numerous objections. In particular, the variable factor of the 'handling-time' cannot be adequately eliminated by mere subtraction, as I have convinced myself by personal trial.

reaction, are alike deficient in mental efficiency as represented by the class-standing." Now, would it not be plausible to modify this inference in this way:—the most intelligent children, as indicated by class-standing, are most able to follow instructions, and therefore to approximate the norm, while the less attentive children are erratic and prone to yield either premature or delayed reactions? This, it seems to me, is the true solution of Bagley's results.

Gilbert[1] tested school children of various ages, using a visual discrimination reaction (react to blue, not to red) and also a simple visual reaction. The results of the two tests were closely similar and may be considered together.

In his recent article upon the objective measurement of "General Intelligence,"[2] C. Spearman criticises Gilbert for not working out the correlation between reaction-time and mental ability by more exact formulæ, and explains the low figure which can thus be actually found as the result of the unfavorable conditions under which the procedure was made, the heterogeneity of the reagents, etc., with the final conclusion that the true correlations must be very much higher than those actually observed.

It seems to the writer that such criticism fails to reach the heart of the matter. How would Gilbert's results stand in the face of the rigorous standardization of conditions which we have found necessary in the laboratory? Is there any value in computed correlations from data which are themselves so loosely gathered?

For instance, it will be recalled that Gilbert argues at length for the use of the median-value versus the arithmetical average. He says that in a series such as 240, 230, 220, 250, 480, 430 sigma, etc., the median-value method is more satisfactory because the large variants, like 480 and 430, do not so much influence the final value, while their force is directed, as it should be, toward increasing the size of the mean-variation. But in the light of all we know of the conduct of the experiment in the laboratory, such variants as 480 and 430 ought not to be admitted to calculation under *any* method, for they are clearly not *bona-fide* measurements of the reaction-consciousness: they do not stand for, or measure, the same psychophysical process as the other values cited. Why, then, should they be classed with them for further treatment? In the laboratory they would be discarded,—not by the experimenter, but by the observer's introspective control. Inspection of Gilbert's tables shows that the median value is, well-nigh uniformly,

[1] *Studies Yale Psych. Lab.*, II, Nov., 1894, 40.
[2] This *Journal*, XV, 201 ff., esp. 280-1.

from 15 to 20 sigma shorter than the arithmetical average: this indicates the presence of a few large values like those mentioned in nearly every group of tests.

Again, suppose we compare the actual times with those given in the laboratory. The values for the older children range, in some groups, from 170 to 180 sigma (arithmetical) and from 155 to 172 sigma (median value). These values, then, are *less* than the *muscular* reaction-times of practiced laboratory observers. But can we stop here? Can we even suppose that Gilbert's school children were mainly giving muscular reactions? Despite the possible suggestion of reacting as quickly as possible, I feel sure that this was not the case, and for three reasons:

(1) The rapid, semi-automatic reaction is nearly always the result of practice and is not apt to appear in a short first series.

(2) It is well known that visual reactions, more than those of any other modality, favor the sensorial type in an unpracticed observer, on account of the necessity of sensory accommodation for the reception of the stimulus.

(3) Gilbert took express pains to foster the sensorial attitude in order to guard against premature reactions. This he did by placing the discrimination reaction before the simple reaction in the order of tests, in order thereby to engender a habit of watching the stimulus.

We may, therefore, look with some suspicion upon such curiously small average times as those recorded by Gilbert.

And, again, if we add to this that Gilbert's mean variations are far above the 10% limit allowed by the laboratory, we are assured that such values are practically useless as measurements of reaction-times. They illustrate very well what I have in mind as the divergence of the anthropometric from the laboratory standards of conducting psychological tests.

But, even assuming that these values represented trustworthy reactions, what evidence have we that they are correlated with mental ability? When we are told that 'bright' children react more quickly than 'dull,' though less difference can be made out in the case of the 'average' group, we find the quantitative support is afforded by the figures 207, 213 and 224 for the three groups. And how much within the limits of certainty are we when the statement that "the brighter the child the more quickly he is able to react with discrimination and choice" is supported by the values 401, 402 and 420 sigma for bright, average and dull children respectively?

From a gradual increase in the rapidity of the reaction with age, even if established, we cannot argue that the increase is attributable to increased general mental ability. Mental growth and mental maturity must never be confounded with mental ability.

The work of Wissler[1] presents an exhaustive treatment of the results of various physical and mental tests of students of Columbia University as compared with one another and with the class standings. The conditions under which reactions were taken in this series are evidently midway between the best laboratory conditions and the looser conditions of Bagley and Gilbert. The reagents are very much of an age, are presumably all of intellectual ability above the average, while the external objective conditions were those used in the psychological laboratory. On the other hand there is no record of introspective control. The tests were few in number (three to five from a set of five), and evidently somewhat hurriedly taken, as Spearman has surmised.[2] Wissler, however, shows clearly that reaction-times are useless as indications of mental ability or even, what is more surprising, of general alertness or readiness of thought or action. The marking of A's, the naming of colors, the execution of rapid movements, and the recording of simple associations (all tests which put a premium upon rapidity and general psychophysical facility) fail to show any significant correlations with one another,—a conclusion, as the author remarks, "out of harmony with all general belief." And the relation of reaction-times with class-standings is merely that of chance.

If our arguments have not gone astray, we seem bound, in the light of our discussion, to conclude that the reaction-test is quite without significance as a measure of mental ability (save in so far as a small mean variation might indicate a certain steadiness in the control and direction of attention). We see that the reaction-time of any observer is determined by a large number of more or less independent factors, and that, when these factors have either been eliminated or controlled, as they are in laboratory procedure, we have left no residuum of individual variation that can be turned to account in estimating the observer's general intelligence or mental ability.

May it not be, or rather, is it not certain, that many other psychophysical tests which have been employed in a rough and ready way, often by inexpert experimenters, especially in the tests conducted upon school children, are quite as likely to be vitiated by factors similar to those we have considered?

At the risk of a digression, I may cite, as an instance, the test of pitch-discrimination which Spearman[3] has just advocated as a universal and ready means for the estimation of "General Intelligence." After conducting tests in pitch-discrimination in the psychological laboratory under all sorts of con-

[1] The Correlation of Mental and Physical Tests, *Mon. Suppl. Psych. Rev.*, June, 1901.

[2] *Loc. cit.*, 283.

[3] *Loc. cit.*

ditions for more than three years, I am positive that the use of a monochord to measure pitch-discrimination, as was done by Spearman, is far too rough a method to use for this purpose. What, for example, shall we infer in regard to the general intelligence of the young lady reported by me in a previous study[1] who was totally unable to judge a half-tone when struck with the slightest unevenness of accent upon the piano, but who could, after a little practice, give a value of S = 0.52 vibs. when the Stern tone-variator was used, and the influence of disturbing secondary factors thereby eliminated? I cannot forbear quoting my previous summary of these tests of pitch-discrimination: "A typical unmusical observer, when placed under proper conditions, may discriminate pitch differences of less than three vibrations correctly in 75% of the tests, but if the stimuli are of relatively low pitch, if they are given without any preliminary 'warming-up,' if the time-interval between them exceeds four seconds, if they are given too briefly or in too quick succession, if they are of unequal intensity, or if they are presented simultaneously with one or more other similar stimuli, then discrimination becomes either difficult or quite impossible, and it may then remain impossible even when D is represented not by a few vibrations, but by musical intervals of one or two octaves and more."

We have here, evidently, simply another illustration of the proposition which I have attempted to establish in regard to the reaction-experiment, viz:—the results of a psychophysical test are determined very largely by the conditions under which the test is conducted.

Summary.

(1) The reaction experiment, as conducted in the psychological laboratory, is primarily a qualitative experiment for the synthesis of the action-consciousness. When due regard is paid to the objective conditions, to practice, and to the direction of attention, most observers can readily be trained to adopt any of the three types of attention. And, for a given direction of the attention, the quantitative results are uniform for all observers.

(2) The reaction experiment of the 'anthropometric' type, as conducted upon school children, is a rough and ready quantitative experiment, which has yielded results of very questionable value as adjudged by the laboratory standards. The constant individual differences between groups of children, which have been assumed in some studies to indicate differing grades of mental ability or general intelligence, are largely the products of faulty experimental conditions, and cannot, in any event, be

[1] This *Journal*, XIV, 297-304.

referred, either in theory or practice, to constant individual differences in mental ability.

(3) This examination of the reaction-experiment suggests that many other experimental studies of school children have been vitiated by the neglect of those cautions in procedure and interpretation that laboratory practice has taught. As an illustration, the pitch-discrimination of an unmusical observer is shown to be subject to wide variation when the test-conditions are modified but slightly.

(4) The outcome of the reaction-time test (and, indeed, of any psychophysical test) upon school children will, furthermore, depend not only upon the objective conditions of the test, upon the nature of the instructions given, etc., but also to an appreciable extent upon the ability of each child to understand and carry out these instructions. When, therefore, a test is affected in this way, any assumed correlation between the quantitative results and the general intelligence of the group of children tested is, in reality, but a correlation of general intelligence with itself.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF MICHIGAN.

CONTRIBUTED BY W. B. PILLSBURY.

VII. THE EFFECT OF STIMULI UPON THE LENGTH OF TRAUBE-HERING WAVES.

By C. E. GALLOWAY.

In two papers, published about three years ago, Dr. Slaughter[1] and Mr Taylor[2] showed that fluctuations of the attention stand in close relations to certain physiological rhythms, chiefly the Traube-Hering waves and respiration, and that they are considerably affected by sensory stimulation. These results suggested a series of experiments on the effect of stimuli on the Traube-Hering waves with a view to finding whether or not it coincided with the effect of stimuli on the fluctuations of the attention. The general aim was to secure, if possible, additional evidence for the theory that the two processes have the same physiological basis and also to determine the effect of pleasant and unpleasant stimuli upon one more physiological process. When these experiments were well under way Prof. Pillsbury, who was then investigating the relation of the attention waves to fatigue,[3] suggested a parallel investigation of the Traube-Hering waves to determine whether or not they showed a "diurnal periodicity corresponding to that noted in the attention waves." Accordingly a series of plethysmographic tracings were taken from five of the subjects used by Prof. Pillsbury in his experiments and the results compared with those which he had obtained. A very close correspondence between the two series appeared, but certain apparently anomalous results will require discussion, and reference to the earlier work will be necessary. It seems best, therefore, to begin with a brief summary of the conclusions which may be drawn from the results set forth in all three of these papers so far as they have any bearing on our present work.

Dr. Slaughter's conclusion from his experiments means that "the fluctuations of the attention are in close connection with at least three physiological rhythms." In the great majority

[1] *Am. Jour. of Psy.*, Vol. XII, 313.
[2] *Ibid.*, 335.
[3] *Am. Jour. of Psy.*, Vol. XIV, 277.

of cases they follow the Traube-Hering waves; in some subjects the respiratory rhythm predominates, while in a third class the important thing is a much longer rhythm which is seldom represented in the volume tracings. Since this third wave was found in but one subject, and as in our later experiments Traube-Hering waves of about the same length not infrequently appeared singly or in short series among waves of medium length, it seems needless to set it in a class by itself; and the important result reached is that the periods of fluctuation stand in close relation to the vaso-motor and respiratory processes. In explanation of this relation Slaughter suggested that the activities of the vaso-motor and respiration centres "in some may reinforce the functional activity of the sensory cells." This theory was suggested by the discovery that voluntary effort caused a marked increase in the efficiency of attention, as measured by the ratio of the period of visibility to that of invisibility, while the total wave was shortened; and by the fact that "respiration waves are found in blood pressure tracings and that both vaso-motor and respiration effects are found in the rate of the heart." In the latter the effect noted is probably to be explained by the overflow of impulses from one centre to another while in the former there seems to be a similar reinforcement of the sensory centres by irradiation from the motor centres. The shortening of the total attention wave as a result of voluntary effort will be discussed later.

Last year Bonser,[1] working independently and in ignorance of Slaughter's results, confirmed the parallelism between fluctuation of the attention and the respiratory and vaso-motor rhythms. In five of the seven records which he reproduces the attention waves follow the respiratory rhythm though the vaso-motor waves are quite distinct in the pulse tracings. In the other two the periods of visibility begin, as a general rule, shortly after vaso-constriction commences and continue well into the following vaso-dilation. Strangely enough Bonser makes no mention of the influence of respiration.

The object of Taylor's work was to "throw some light on the much disputed question as to whether the attention waves are of central or peripheral origin" by ascertaining the influence of certain sensory stimuli on the fluctuations. The minimal stimulus was obtained from a Masson disc and the fluctuation recorded by a Marey tambour on a horizontal drum. At each observation a normal record was taken and then a record with stimulation, usually by an induction current, though both pleasant and unpleasant olfactory stimuli were sometimes used. The fluctuations were measured and averaged and the length of

[1] Psychological Rev., Vol. X, pp. 120 ff.

the total wave as well as the attention efficiency under the influence of stimulation compared with the normal length and efficiency. Of the three subjects used the first gave a decrease of 3.6 sec. in the length of the total wave under unpleasant stimulation and an increase of .4 sec. under pleasant stimulation. The attention efficiency varied directly with the length of the total wave. With the second subject all stimuli, whether pleasant or unpleasant, increased the length of the wave an average of 1 sec. and heightened the efficiency. With the third subject the length of the total wave was uniformly increased but the efficiency was sometimes increased and sometimes the reverse. In the case of the first subject the respiration waves varied with the attention waves while in the other cases the breathing waves were shortened and the attention waves lengthened.

In the face of these apparently contradictory results we must agree with Taylor that each subject must be studied separately. The fact of wide individual variations is shown clearly. It seems perfectly plain that, so far as the effect of sensory stimulation on the length of the attention waves and an attention efficiency is concerned, every individual is a law unto himself. It is no less plain on the other hand, that in most cases stimulation causes a lengthening of the total wave and along with this an increase in efficiency. The problem involves the study of a set of functions which are easily disturbed by any kind of interference and which are bound up with plastic structures in which wide individual variations on the one hand and the establishment of fixed tendencies on the other are to be expected. Thus the impossibility of discovering rigid and narrow rules which are valid for all subjects is evident at the start; but it is possible to show the existence of general laws holding throughout the various types to which the particular cases may be reduced.

For further evidence of the necessity of this method we may turn to Prof. Pillsbury's work on attention waves and fatigue. In these experiments the fluctuations were recorded on the horizontal drum used by Taylor. Four records per day were taken from each subject with the exception of one man whose afternoon record was omitted. The records were taken at about 9:00 A. M., at noon, at about 2:00 P. M., and at 6 P. M. It was found that the length of the attention wave and the attention efficiency have a diurnal periodicity relatively constant for each individual while the curves of different individuals vary widely, and further that this curve indicated the type of worker to which the subject belonged. It appeared, also, that in every case "the greater length of wave corresponds to the time of greatest attention efficiency," which confirms the general con-

clusion we drew from Taylor's work. The fact that this relation held for all six of the subjects used, and through a series of experiments extending for most of them over a considerable period of time, is suggestive. In connection with the previous work the results would seem to point directly to the central origin and control of the fluctuations.

Summing up the work thus far we may conclude:

1. That in most subjects the attention waves stand in close relation to the Traube-Hering waves, while in a minority of cases there is an equally well defined coincidence with the respiratory rhythm.
2. That voluntary effort and sensory stimulation increase attention efficiency in nearly every case.
3. That in the majority of cases the periods of greatest efficiency coincide with the periods of greatest wave length while in a few cases the reverse is true.
4. That similar individual variations occur in the effect of fatigue and sensory stimulation on the length of the attention wave while this effect is constant for particular individuals.
5. That the fluctuations show for each subject a diurnal periodicity of efficiency and wave length peculiar to that subject.

In view of these conclusions the necessity of further work on the Traube-Hering waves is plain. If Slaughter's theory has any basis in fact we should expect that in the great majority of subjects the vaso-motor waves would be affected by voluntary effort, by sensory stimulation and by fatigue in precisely the way in which the attention waves are affected by the same influences. More exactly we would expect: (1) that voluntary effort would increase the length of the Traube-Hering waves in those subjects whose records show an increase of attention efficiency to correspond to an increase in the length of the total wave, and that the opposite effect would be produced in those subjects whose records show the reverse of this relation; (2) that sensory stimulations would have the same effect on the length of the Traube--Hering waves as on the length of the attention waves, *i. e.*, would increase it in nearly every case; (3) and that the Traube-Hering waves would show *for each subject* a "diurnal periodicity corresponding to that noted in *his* attention waves." A similar correlation of attention and respiration would be expected in those subjects in whom the breathing rhythm plays the predominant part in determining the fluctuations of attention.

Our experiments were begun with the purpose of determining whether or not these apparent relations can be shown to exist. One difficulty presented itself at the outset. No direct comparison of our results with those obtained by Slaughter and

Taylor was possible, since we were obliged to use different subjects, and while Taylor's subjects showed very wide individual differences, our own, as will be seen, presented far less distinct differences of type. It will be shown later, I think, that this difficulty is not so serious as it first seems. On the other hand a very close correlation of our results on the diurnal changes in the length of the Traube-Hering waves with those on the changes in the attention waves was possible, for the same men were used as subjects throughout. This is important, for it enables us to emphasize the fact of individual differences on the one hand and the close agreement of attention and vaso-motor or respiratory rhythms on the other, and on this basis to explain the apparent opposition between our results and those previously reached.

In our work upon the effect of stimulation on the length of the Traube-Hering waves the tracings were taken on the drum of an ordinary vertical kymograph. The pulse tracings were recorded by means of the delicate piston-recorder attached to the finger-plethysmograph or by the Hallion-Comte plethysmograph in connection with a Marey tambour. A pneumograph attached to a Marey tambour registered the changes in respiration and the time was recorded by a Jacquet chronograph. The pointers were set in a vertical line so that any disturbances in the respiration and pulse tracings could be carefully compared. The time required for a revolution of the drum was about five minutes and each record lasted through two revolutions. During the first an effort was made to eliminate every disturbance and secure a perfectly normal tracing; during the second the stimulus was applied. This stimulus was commonly an induction current passed through the free hand, but with three of the subjects pleasant and unpleasant gustatory and olfactory stimuli were sometimes substituted. The Traube-Hering waves were projected on a straight line and there measured. The averages were taken and the effect of stimulus on the length of the wave determined for each record, after which the whole series was averaged for each observer.

The experiments extended through the greater part of the year. Five subjects were used: Prof. Pillsbury (P), Dr. Wallin (W), Mr. Hayden (H), Mr. Freund (F) and the writer (G).

The results were remarkably uniform. The absolute length of the waves varied considerably for each subject from day to day, but the ratio of the average normal wave to the average wave taken with stimulus was very nearly constant. The following table gives the results in averages.

(The first column gives the subject's name; the second the total number of records taken; the third the average length of

TABLE I.

SUBJECT.	NO. OF REC'DS.	NORMAL.	STIMULUS.	S.—N.
P	27	9.4	11.5	2.1
W	9	10.0	12.4	2.4
H	32	9.6	10.8	1.2
F	14	9.9	12.5	2.6
G	26	10.1	12.5	2.4

the normal Traube-Hering waves for the entire series; the fourth the average length of the wave taken with stimulus; the fifth the average increase in length resulting from stimulation).

In 105 of the 108 records taken the stimulation caused a distinct lengthening of the waves. In two of the remaining three very weak olfactory stimuli were used and no change in the length of the wave resulted. The third record was taken as a curiosity. The subject had complained of headache and nausea before work was begun. Strong vaso-constriction, weak pulse, and slightly shortened Traube-Hering waves resulted from the stimulation with the induction current. In all the other cases the waves were lengthened, with total indifference to the pleasantness or unpleasantness of the stimulus. The effect of the pleasant and unpleasant olfactory and gustatory stimuli was precisely the same as that of the painful induction current. An abrupt change from a very pleasant to an unpleasant stimulus or *vice versa*, and even several such changes in the course of a single record, produced exactly the same result as the constant stimulus.

Assuming the validity of the conclusions reached by Slaughter and Taylor these are the results we should expect. Taylor's work showed that no general rule for the effect of stimuli on the length of the attention waves valid, for all subjects, can be reached, but that in most cases the wave length is increased and that efficiency generally varies directly with the wave length. Since, as Prof. Pillsbury found, increase in wave length and increase in efficiency do coincide in the case of the five subjects from whom our results were taken, the lengthening of the Traube-Hering waves by stimulation is what must have happened on the basis of the central theory; and Taylor's mixed case, so far from contradicting our results tends indirectly to confirm them.

It will be remembered that Slaughter found that voluntary effort increased the efficiency of attention but shortened the total wave. In the hope of securing further evidence for the central theory Mr. Sherman, who was then doing some work

involving the use of the ergograph, took several pulse tracings to determine the effect of effort on the Traube-Hering waves. The experiments were conducted in practically the same way as those in which the effect of sensory stimulation was being investigated. The observers were Mr. Sherman (S) and Mr. Hayden (H). The records were turned over to the writer and the averages are given in the following table.

TABLE II.

SUBJECT.	NO. OF REC'DS.	NORMAL.	VOLUNTARY EFFORT.	V. E.—N.
S	7	7.5	9.2	1.7
H	10	7.6	9.1	1.5

The effect of voluntary effort was, in each case, to increase the length of the vaso-motor waves which is directly opposed to Slaughter's results. On the other hand he found that the efficiency of attention was increased and in the case of our subjects' increase of efficiency coincides with increase of wave-length. Further there is no reason to suppose that voluntary effort any more than sensory stimulation must affect all persons alike, and it seems probable, as I hope to show, that the effects of effort and stimulation on the Traube-Hering waves must be identical. Finally, and most important, it was found in records previously taken that in both of these subjects voluntary effort increased the length of the attention waves.

It now becomes necessary to bring forward some physiological explanation of this change in the lengths of the vaso-motor waves. The fact that sensory stimulation and voluntary effort affect the vaso-motor waves, in most cases to lengthen them, seems quite beyond dispute, but how this effect is produced is not evident. It is generally supposed that the Traube-Hering waves are due to a periodic excitation of the vaso-motor centre by cyclic changes in the blood pressure at the medulla.[1] Anæmia of the medulla excites the centre and causes a constriction of the peripheral vessels and a consequent rise of the arterial pressure, at which the excitation ceases and the pressure is allowed to fall, anæmia of the medulla results and the round is repeated. The explanation is, however, complicated by the fact that vaso-dilation as well as vaso-constriction is, from the point of view of nervous physiology, an active process. The vaso-motor nerves contain both constrictor and dilator fibres, and a complete Traube-Hering wave involves the activity of both.[2] The fact that vaso-constriction rather than vaso-dilation

[1] Schäfer: Physiology, Vol. II, p. 139. [2] *Ibid.*, pp. 132-134.

follows an excitation of the vaso-motor centre is doubtless due to the fact that the latent period of the vaso-constrictor fibres is but little more than one-third as long as that of the vaso-dilation fibres.[1] It has been shown experimentally that "if the vaso-constrictors and dilators are stimulated simultaneously, the constrictor influence at first overpowers the dilator. The dilator effect, however, appears afterward, for the vaso-dilator fibres are less easily exhausted.[2] That is "the vaso-dilator effect appears after a longer latent period, while it reaches its maximum and disappears more slowly."[3] We would therefore expect to find the Traube-Hering waves in the pulse tracings divisible into a shorter descending section representing the period of vaso-constriction and a longer less steep ascending section representing the period of vaso-dilation. A careful examination of our plethysmographic records was made, and it was found that this relation between the two parts of the wave held for the averages of a large number of measurements, but where only a few waves were measured the reverse of this relation or an exaggeration of it was found. These irregularities were traceable to interference from the respiratory centre and from external stimulations.

The fact that many, if not all, external stimuli cause an initial fall in the volume curve is beyond question, and the only reasonable explanation of this seems to be that here is an irradiation from the sensory centres upon the vaso-motor centre causing immediate stimulation of the constrictor fibres. The overflow of impulses from a sensory to a motor centre is a well established fact of nerve physiology, so this explanation presents no difficulties of theory; and the equally well established fact of the overflow from one motor centre to another accounts for the effect of voluntary effort and demands that this effect be precisely the same as that of sensory stimulation. We would expect, then, on the basis of purely theoretical considerations that the effect of sensory stimulation or voluntary effort on the vaso-motor nerves would be somewhat as follows: There would be an overflow of impulses upon the vaso-motor centre causing increased excitation of both constrictor and dilator fibres. If the activities of the two were precisely alike no change in the volume-curve would take place; but owing to the shorter latent period and greater excitability of the constrictor fibres there would be an initial constriction and a hastening of the constrictor effect which would thus reach its maximum more quickly than under normal conditions, and this part of the wave would be shortened. Since, however, the

[1] Schäfer: Physiology, Vol. II, p. 135.
[2] *Ibid.*, p. 134.
[3] *Ibid.*, p. 135.

dilator effect reaches its maximum and disappears more slowly the decrease in the length of the constriction would be somewhat more than balanced by a proportional or more than proportional increase in the length of the dilation and the effect would be an increase in the length of the total wave. The constrictor effect would be hurried to its maximum and would disappear under the combined influences of exhaustion and the opposition of the increased dilator activity. Then, since the dilator fibres are less easily exhausted and have no further opposition, vaso-dilation would continue beyond the normal period, its maximum effect is increased and consequently the time required for its disappearance is greater than when no reinforcement was present. The relative change of length would probably be slightly greater in the period of constriction, but even if this were true the final result would be an increase in the length of the whole wave owing to the greater length of the normal dilation. Further examination of the pulse tracings showed that in spite of many irregularities and variations the increase in wave-length under stimulation was due to an increase in the length of the period of dilation while the length of the period of constriction was decreased. We were, of course, obliged to use averages, no direct comparison being possible. The general effect is well brought out in Table III taken from a somewhat exceptional record.

TABLE III.

N.	Nd.	Nc.	S.	Sd.	Sc.
10	6	4	11	8	3
7	4	3	12	8	4
12	7	5	9	7	2
10	7	3	11	6	5
10	6	4	11	9	2
10	6	4	11	6	5
8	5	3	14	10	4
9	6	3	8	6	2
8	5	3	11	9	2
12	7	5	12	9	3
7	4	3	10	8	2
11	6	5	10	8	2
9.5	5.7	3.8	10.8	7.8	3

(The column headed N gives the length of 12 successive waves from the middle of a normal tracing. The columns headed N d and N c give the periods of vaso-dilation and vaso-

constriction in each wave. The columns S, Sd and Sc give the same for the middle portion of the corresponding record taken with stimulation. The averages are at the bottom of the column. It will be seen that the lengthening of the waves averages 1.3 sec. The average increase in length of the dilation is 2.1 sec., while there is a decrease of the difference, .8 sec., in the length of the constriction.)

It now seems evident that any reinforcement of the medullary centres from the cortex, whether from sensory or motor activity, should produce an increase in the length of the Traube-Hering wave. In any case the source and character of the stimulus should make no difference in the effect. Against these conclusions, however, we have Slaughter's statement that voluntary effort produced a decrease in the length of the attention wave. Two explanations of this may be suggested, but an examination of his records and of pulse tracings from the subjects from whom his results were obtained, would be necessary to decide which is to be accepted. It is possible that the respiratory rhythm determined the fluctuations of attention in the cases in question and our records show that in nearly every case reinforcement of the medullary centres quickens respiration. It is also possible that in these subjects there was an unusually close balance between the activities of the constrictor and dilator waves. The records taken from H show a very close balance between the length of the constrictor and dilator periods, and reference to Table I shows that the increase of wave length caused by stimulation is only about one-half as great as in the other four cases. That in some individuals the opposite of the usual relation holds is uncertain but not impossible or even improbable.

We pass now to a consideration of the daily rhythm of the Traube-Hering waves. The apparatus was the same employed in the investigation of stimulus effects. Four records per day were taken at the hours on which Prof. Pillsbury had taken his records in the experiments described above. Each record lasted from five to ten minutes and showed from fourteen to sixty-two Traube-Hering waves. The subjects were those from whom the results of Table I were obtained and from whom Prof. Pillsbury's records had been taken. In the cases of all the subjects except F some months elapsed between the times when the records for the daily rhythm of the attention waves and those for the daily rhythm of the vaso-motor waves were taken. The comparison must, therefore, be of averages and a parallelism of tendency is all that can be expected. Where the attention waves of any subject show a uniform daily periodicity a corresponding periodicity should appear in the Traube-Hering waves, but the absolute lengths, even when the averages are

used, would almost certainly differ. The following tables give the results which are obtained and show the general correspondence expected. The attention measurements in the fourth column are copied from Prof. Pillsbury's tables in his articles on Attention Waves as a Means of Measuring Fatigue.[1]

[1] *Am. Jour. of Psy.*, Vol. XIV, 277.

TABLE IV.

P.

TIME.	NO.	T.-H.	ATT.
9.00 A. M.	106	10.0	9.6
Noon	88	9.2	8.5
2.00 P. M.	102	9.0	8.2
5.30 P. M.	124	8.6	7.5

TABLE V.

W.

TIME.	NO.	T.-H.	ATT.
9.00 A. M.	107	9.9	5.4
Noon.	116	10.4	6.3
2.00 P. M.	114	9.0	
5.30 P. M.	98	8.3	5.1

TABLE VI.

H.

TIME.	NO.	T.-H.	ATT.
9.00 A. M.	367	9.5	9.4
Noon.	316	8.4	8.6
2.00 P. M.	483	8.9	9.1
5.30 P. M.	252	8.0	8.4

TABLE VII.

F.

TIME.	NO.	T.-H.	ATT.
9.00 A. M.	80	9.4	18.9
Noon.	96	9.0	18.2
2.00 P. M.	87	6.3	13.4
5.30 P. M.	111	9.5	18.5

TABLE VIII.

G.

TIME.	NO.	T.–H.	ATT.
9.00 A. M.	174	8.8	8.9
Noon.	160	8.9	8.9
2.00 P. M.	203	8.4	8.4
5.30 P. M.	178	8.6	8.4

(The first column gives the time of day when the records were taken; the column headed No. gives the total number of waves measured; that headed T—H the average length of the Traube-Hering waves; and that headed Att., the corresponding measurements of the attention waves.)

In Table IV the general correspondence between the vaso-motor and attention rhythms is evident in spite of the greater length of the vaso-motor waves. In explanation of this difference in length it is enough to say that the records for the attention waves were taken in the middle of July while those for the Traube-Hering waves were taken in mid-winter and under very different conditions. Why the difference should be in this direction rather than the opposite it is impossible to say, but that there should be some difference was to be expected. What is significant for us here is the fact that there is a general decrease in the length of both the attention and the Traube-Hering waves from morning to night, a parallelism which can scarcely be accounted for save on the assumption of a common physiological basis.

In table V two interesting features are presented: (1) There is the evident correspondence between the attention and respiratory rhythms, shown by the very short attention waves; and (2) the fact that these waves show a definite daily periodicity which corresponds with that of the Traube-Hering waves. There is an increase in the length of both attention and Traube-Hering waves until noon and a decrease in the afternoon. This parallelism is readily explained by the fact that in the normal records the vaso-motor and respiratory activities seem always to vary in the same direction.

In Table VI the correspondence is much closer than in either of the two preceding cases. Not only are the absolute lengths nearly the same but the peculiar daily periodicity of the one is very accurately repeated in the other. Attention and vaso-motor waves show a decrease in length in the morning, a rapid recovery during the noon rest and again a marked decrease through the afternoon.

Table VII presents a new complication. The attention waves are about twice the length of those taken from the other subjects and almost exactly double the length of the Traube-Hering waves. As the two series were in this case taken within a few days of each other a fairly close correspondence of the absolute lengths would be expected. As for the two-to-one relation it seems probable that there is a summation effect at its basis, influenced also, perhaps, as the records suggest, by coincident reinforcement from the respiratory centre. But here, again, the important fact is that there is a very definite and constant relation between the two series indicating a common physiological basis.

In Table VIII the parellelism is so close that it may be allowed to pass without comment. The absolute lengths, as shown by the averages, are almost exactly the same for all four periods.

The significance of the results shown in these tables is unmistakable. The fact that the attention and Traube-Hering waves have the same daily variation is in direct line with Slaughter's conclusions and is one more bit of evidence that the two rhythms are causally related or at least have a common basis. Even if Slaughter's explanation of the relation between them is incorrect the relation itself would seem to be pretty thoroughly established.

It would seem to be of interest in the general controversy as to the effects of pleasantness and unpleasantness upon circulatory phenomenon, that all stimuli of any intensity have exactly the same influence upon the length of the Traube-Hering waves. Evidently here there is no trace of an opposed relation between pleasantness and unpleasantness.

SUMMARY.

1. Traube-Hering waves are increased in length for each of five subjects by stimuli whether pleasant or unpleasant.
2. Muscular contraction, for two subjects, increased the length of the waves.
3. The daily variation in the length of vaso-motor waves is in the same direction as in the attention waves from the same subjects.

If we bring these results into connection with the effect of similar stimuli on attention waves, the argument would run, that Traube-Hering like attention waves are influenced by stimuli of all kinds, and that the universal direction of change for all our subjects was the same as for the majority of those whose attention waves have been studied. That there is great probability of individual variation, and that

subjects will be found for whom stimuli of suitable strength would quicken the rate of the Traube-Hering wave, is not unlikely. Only simultaneous records of attention and Traube-Hering waves can decide this point.

The similarity in the dirurnal variation and the close approximation in length of the two sorts of waves in three subjects, in spite of the different times at which the two investigations were carried on, make very strongly for the common physiological basis of the processes.

VIII. The Effects of Closing the Eyes upon the Fluctuations of the Attention.

By Bertha Killen.

Münsterberg's peripheral theory takes the stand that the fluctuations of the attention are due to the fatigue and recovery of the accommodation mechanisms in the sense organ. He made a number of elaborately varied experiments upon the sense of sight. His method of experimentation was to fixate the gray rings of the Masson disk and record the fluctuations in series upon a white kymograph drum by means of a tambour and pencil. The general average of the normal fluctuations was first established at 6.9 seconds. He then varied his experiments by use of a "*prismatische lorgnett*" by means of which the field of vision was moved slightly to the side, requiring a quick movement of the eyes in order to keep the fixation continuous. In case the prisms were held continuously before the eyes, no very great change in the fluctuations would appear. But when the glass was interposed at intervals of two seconds it was found that the fluctuations could be lengthened from 11 to 14 seconds. In the next series, a sound was made by an assistant, every second, which caused the subject to close the eyes quickly for a moment, making a scarcely noticeable break in the fixation. Under these circumstances a decrease to entire vanishing never took place. But when the disk was covered at intervals by a screen the fluctuations went on unhindered.

In view of the fact that Pace recently used these results as a proof of the peripheral or retinal origin of the attention waves, it seemed worth while to repeat them to test their accuracy. Münsterberg argues from these results that the momentary rest permitted the eyes to recover from fatigue.

The subjects were Miss Udell (U.), Miss Barnes (B.), and Mr. Wright (W.), all research students in psychology and were careful, experienced subjects.

The experiments were carried on from September to February, 1903-04. In the case of U. and B. a normal and a modified

record were taken once a week at the same hour; with W., a normal was taken once a week Tuesday or Thursday and a modified record each Tuesday and Thursday, at the same hour.

The manner of experiment was as follows:

The subject fixated a Masson disk which was revolved by an electric motor and showed one dark ring. The subject sat at a uniform distance of about four feet from the desk. To get constant illumination, the disk was placed in a dark room lighted by electricity. A key was pressed at the appearance and released at the disappearance of the ring by the subject's right hand. The make and break were registered electrically on a smoked drum which permits a record of about ten minutes in length to be made. The drum with its motor was placed in a room 100 feet away. The normal record requires no comment. The modified record was taken in the same way with the exception that the eyes were closed every second on the beat of the metronome.

For B. and U. there were 20 normal and 20 modified records each; for W., 20 normal and 40 modified records.

The results vary and differ with each subject. The following are the tables of results:

Average length of period of visibility:

	B.	U.	W.
Normal,	3.8 sec.	5.4	7.6
Closed Eyes,	4.6	5.6	7.

Average length of period of Invisibility:

	B.	U.	W.
Normal,	3.2	2.8	3.4
Closed Eyes,	4.2	2.4	3.2

Ratio of Invisibility to Visibility. Each record was divided into five parts.

						Ave.
B.						
Normal,	.93	1.07	.92	.71	.60	.85
Closed Eyes,	.82	.87	.85	.95	.104	.91
U.						
Normal,	.71	.47	.48	1.56	.51	.75
Closed Eyes,	.54	.46	.40	.46	.40	.45
W.						
Normal,	.36	.50	.54	.51	.57	.50
Closed Eyes,	.41	.35	.61	.54	.64	.50

In the case of B., closing the eyes once every second increased the wave of visibility .8 seconds and the period of invisibility one second. With U., the period of visibility was increased two seconds and the record of invisibility decreased .4 seconds. With W., neither the average visibility or invisibility show the increasing tendency. A study of the different stages in the record, however, shows there is a decided

increase in the third period and a slight increase in four and five, it is evident that the voluntary effort first inhibits, then reinforces the perceptive processes. These two influences happen just to cancel each other and the general average is the same for each.

In the case of B., closing the eyes increased the relative efficiency of the attention wave; with U. the effect was a decrease of the efficiency; for W., there was first a decrease then an increase in the efficiency. These individual differences can be explained consistently by the fact of individual differences. Taylor,[1] in his article on "The effect of Certain Stimuli Upon the Attention Wave" has shown that with the same person one stimulus may increase the efficiency of the attention while a stronger would decrease it. In these experiments the stimulus was uniform but would affect different subjects as stronger and weaker. To U., they seemed stronger than to the others.

With W., the proportion of black in the ring was increased until the fluctuations disappeared entirely during the latter part of the record in the closing eyes series. This was found when there was 1/50 part black in the ring. In the normal record the fluctuations still appeared as follows:

Wave of Invisibility 1.8 sec., Visibility 5.2 sec.

The disk used for the other records with W. had 1/75 part black; for U. and B., 1/92 part black.

It would seem, then, that Münsterberg's results are not valid for all people or for all intensities of stimulus or degrees of stimulus differences. What does happen is that there is for certain individuals, probably those of a phlegmatic temperament, a tendency for voluntary effort, as for any stimulus, to reinforce the sensory processes involved in seeing the gray ring, and it chanced that Münsterberg hit upon exactly that intensity of stimulus which could be made supra liminal by the reinforcement. We have beeen able to hit upon the same intensity for W. For B. we find the same tendency but the stimulus difference was too far below the limen to be made continuously supra liminal. For U., on the other hand, there is a tendency toward decrease of efficiency. It seems very much easier, then, to explain both Münsterberg's results and our own by the central reinforcing than by a peripheral theory.

Summary.

1. Closing the eyes for a moment at regular intervals increases the length of the attention waves and decreases the time of visibility.

2. The slight stimulus of closing the eyes has a tendency to increase the efficiency of the attention waves.

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[1] *American Jour. of Psy.*, Vol. XII, p. 335.

THE TENDENCIES OF EXPERIMENTAL PSYCHOLOGY IN ITALY.[1]

Critical Note by Dr. GIOVANNI CHIABRA, Professor of Philosophy in the Academy of Lanciano (Chieti).

Here in Italy, where the interest in experimentation arose during the brightest period of the Renascence, chiefly with the great Galileo, the experimental study of mental phenomena is assiduously pursued. We have three laboratories of Experimental Psychology: one at Turin, in the Institute of Physiology, directed by Prof. Angelo Mosso; another at Rome, established and directed by the anthropologist, Prof. G. Sergi; a third at Florence in the Institute of Higher Studies (Professional and Training School), of which Prof. Francesco De Sarlo, the eminent psychologist and philosopher, now physician and surgeon in the royal army, is director. I am seizing this present opportunity to give a brief note on the actual state of Experimental Psychology in Italy; reserving for another paper the mention of other tendencies that have able representatives among us,—men like Senator Carlo Cantoni, recently appointed honorary professor in the University of Königsberg, in recognition of his eminence in Kantian studies; or the professors Fillipo Masci, Roberto Ardigò, Francesco Bonatelli, and others. The main purpose of this brief essay is to trace the two currents of psychological thought, now dominant in Italy, which, in a general way, correspond to the schools of Münsterberg[2] and Wundt.

[1] Translated by Mr. H. C. Stevens, Cornell University.

[2] H. Münsterberg, as is well known, starts out, as does the Wundtian School, from experimental psychophysiology; but he arrives at conclusions different from those of Wundt, in that he reduces all the complexity of mental life to sensations alone. Moreover, in his action theory (Aktionstheorie), he considers the impulse solely in the mechanical aspect of the reflex act, subordinating it, therefore, to physiological laws. In opposition to Wundt who admits the duplicity of causality, with respect to internal and external experience, Münsterberg maintains that psychical causality is not valid; that, for example, the ultimate problem of the psychology of the will consists in determining what excitations of cortical centres are necessary, in order that the sensations which arise in consequence combine in a way to constitute an act of will. But Münsterberg's theory is not founded on positive facts, nor is it sustained by physiology; it is, furthermore, too schematic, and it is contradictory to psychological experience.

I.

The representatives of the first tendency expressly postulate a complete correlation between mental organs and mental functions. Experimental Physiology, they say, has bridged the gulf between the two provinces of fact, although up to the present time it may have explored only a very limited field, in which there is much doubt and uncertainty. But the proof that can be given for other organs and functions is still lacking in the case of the brain and mental phenomena. It is true there have been some innovators in histology who believe that the solution of the psychological mystery is to be found in the cells stained by Golgi's method. In order to gain a vantage ground, they have called the pyramidal cell of the cerebral cortex, "psychical cell" or "cell of volition." They have asserted, further, that this cell synthetizes our conscious activity by expressing it in a voluntary act, for that reason acquiring the highest morphological differentiation. But serious criticism soon showed that they had reinstated an error of the old metaphysics, habited in modern garb. The hypothesis of a "cell of volition," of an "archicell," of a "pontifical cell" was of the same sort as that of the "seat of the soul." The soul, thanks to the advances of physiology, was always shifting from one seat to another: from the pineal gland, which is known to be an atrophied centre of vision, from the ventricles of the brain, from the centrum ovale, from the corpus callosum. So that, if the hypothesis of one seat were still tenable, the soul would have to be lodged in all the psychical centres, and even in the whole of the cerebrospinal nervous system. The same school declares that if it were true that physiological psychology presupposes the theory of a nerve motion, which has been named neurokyme, it must be admitted that the nature of this nerve motion is wrapped in profound obscurity; but that even if the physiology of the central nervous system had got beyond the tentative stage, and if the theory of neurokyme had been developed as far as the physics of vibrations, and if, further, mechanical formulæ had been derived, representing the masses, velocities and positions of all the cerebral elements of a given moment, still, one would merely know how the energy of the stimuli had been transformed into an organic molecular motion by means of the potential energy accumulated in various organs of the nervous system; but in this physical process there would still be no trace of a mental process. Accordingly, several authors concern themselves with the mode of behavior of the nerve cell and of the cells of the sense organ under different conditions of activity and repose. It seems, moreover, that marked changes, in both the nucleus and protoplasm, have been

brought to light. There are, too, some workers who attempt to demonstrate minute changes in the cells, in different pathological conditions of the nervous system. But all of these studies offer only promises, beneath which one seems to hear the whisper of something like the hope which always flourishes where science flourishes,—though in reality the attempt is fallacious—the hope of founding all the phenomena of life on these chemical and physical phenomena, of pervading physiology through and through with chemistry and physics. In the actual state of the science, the features common both to the nervous system and to consciousness are enumerated by Höffding.[1] 1. The nervous system serves as the central organ of co-ordination for the different parts of the organism; consciousness unites what is separate in space and time. 2. Change and process are alike conditions for the functioning of the nervous system and for the activity of consciousness. 3. The stimulus does not excite one centre only; but by means of the manifold branchings of the nervous system, it evokes a series of excitations in centres connected with that first affected. In the same way, the effect of a sensation is extremely complex, since it has the power to recall a great number of ideas. 4. The reaction time, or physiological time, stands in direct proportion to the complexity of the mental operations. 5. Corresponding to the opposition between the active and passive aspects of thought, there are the sense organs and afferent nerves on the one hand, and the nerve centres and efferent nerves on the other. But except for these conclusions, showing the attempts of those who would make psychology "the physics of human thought," no physiologist on the basis of all the wonderful advances of his science would be able to deny that there is a gulf between the molecular state of the brain corresponding to a thought and the thought itself. Supposing for a moment, at least, that physiological psychology had achieved such progress as to be able to express with mathematical precision, the velocity, direction, composition and reaction of molecular motions, for every volition, emotion and thought, still, however much faith one has in science, the physiological series, for thought, would simply be a system of signs analogous to those of language. Unless, indeed, by starting out from an *a priori* proposition, with the method that has been so much condemned in Scholasticism, our psychologists mean to deduce a *speculative* doctrine of the relations between the matter of the brain and the manifold manifestations of the mental life, not content with the pure and simple conclusion to which all the analyses and all the research of contemporary science have come. At the same time, apart from

[1] Psychology, pp. 62-66.

this prejudice which tends to reduce the spiritual entity to a mechanical law, as though man were no more than a mass of muscles and nerves, irrigated by blood, important researches have been accomplished in this field of psychology, which with strictness of terms may be called physiological psychology. Angelo Mosso, especially, may be mentioned in this connection as a profound observer and investigator. Playing upon the human organism, by his varied physiological appliances, as a musician plays upon his instrument, he seeks to determine by experimental means the organic states and functions which underlie and explain the psychical phenomena revealed to his introspective intuition.

II.

De Sarlo[1] is the representative of the second tendency. He admits that Psychology as a positive science must be taught by the method of positive sciences. A science of fact, he says, can be taught only by means of the demonstration of these facts; and the means of demonstration are observation and experiment. To teach psychology to-day, without some means of demonstration, would be as absurd as on the other hand to found a laboratory of philosophy. And a laboratory of psychology, thanks to De Sarlo's own energetic initiative and to the co-operation of Pasquale Villari and of Felice Eocco, has been recently established in the Royal Institute of Higher Studies.

We hope that a similar change will be made presently, in the other philosophical faculties of the kingdom.

Now the direction which De Sarlo is trying to give to the study of psychology is new, original and, in my opinion, right; it deserves, therefore, a brief note.

Before all, De Sarlo believes that there can be no psychology which is not animated by the principle that "a real subject exists." It is not the business of the psychologist to examine the nature of such a subject, much less, to find a place for it in the system of cognitions, by proceeding from philosophical premises. It belongs to philosophy, and, properly speaking, to epistemology (in this point, he agrees with Wundt and Titchener), to criticise this concept, which may even be false but which is, nevertheless, indispensable to psychology, just as certain prin-

[1] *Cf.* especially, the following parts of the works of De Sarlo. 1. *Experimental Psychology in Germany*, the most important paper, which was published in the Rivista Sperimentale di Freniatria, Vol. XIX, fasc. I; 2. *The Object of Physiological Psychology*, in the Essays in Philosophy, published by Clausen in Turin in 1896; 3. *The Concept of Mind in Contemporary Psychology*, read March 1, 1900, before the Royal Institute of Higher Studies of Florence; published by E. Ducci, Florence; 4. *The Data of Mental Experience*, the latest publication of the Royal Institute of Higher Studies.

ciples are necessary to many other sciences, the atom to chemistry, motion to mechanics, etc., and without them analysis could not be made. An organism can come only from an organism; and, furthermore, even in physiological life, there is something that cannot be reduced to chemical and mechanical principles alone. The evolution of the germ cell shows how an organism requires the postulate of *life* in order to be understood from the physiological point of view: notwithstanding the fact that this postulate is excluded from modern science, on the ground that it would be a vital force detached from the organism. Similarly the concept of mind, understood as a real and active subject, is the postulate of every psychological discussion. It stands for the content of mental life, which has its own principle in itself, in so far as it is something spontaneous and individual. The germ cell is not merely a chemical compound, but it is the potentiality of life; this law holds as much for psychology as for biology. As in the circumference of a circle, every arc exemplifies the law and necessarily presupposes it, so every phenomenon of consciousness, in order to be understood, must be referred to the individual subject of which it is only an expression.[1]

But what is Psychology according to De Sarlo? He gives the following definition of it. "It is that science which has for its object the study of the arousal and course of that particular category of facts or processes which is called psychical." This category is chiefly distinguished from all others by its character of incommunicability. The mental facts are individual; they cannot be transferred to another person. They are something more than that: but when they are studied from the psychological standpoint, they must be considered in abstraction from all the rest of reality, as necessarily referring to an empirical subject,—not the subject with which epistemology has to do, but an organic individual who occupies a determinate position in space and time. It is true that we communicate our thoughts, our ideas and our feelings, etc.,—but we do not communicate directly: rather indirectly, by means of written and spoken words and gestures. "It is impossible to read directly the consciousness of another individual and to feel precisely as he feels at any given moment." At this point, one may raise the question whether this method of viewing the facts of mind, according to De Sarlo, is right. We believe that it is, and we will briefly give our reasons. In order to express our ideas, we are constrained to make use of words; our thought thus assumes a spatial form. That is, the spatial

[1] *Cf.* the admirable lecture, *The Concept of Mind in Contemporary Psychology*, delivered before the Institute of Higher Studies. Florence, 1900, E. Ducci.

distinctions which language forces us to impose on ideas are as sharp and precise as those we impose on physical objects. Now this tendency of ours, to translate non spatial phenomena into spatial terms, thus mutation of quality into quantity, is the cause of all the perplexities arising from certain psychological problems. Indeed, if there exists a contradiction in the formulation of the problem, it is natural that a contradiction, likewise, should be found in the solution. In fact, in the superficial stratum of consciousness which reflects the images of the external world, the phenomena preserve that discreteness, that isolation, which characterizes their objective correspondents (it is precisely to the simple sensation that the theory of association adapts itself); but in the deeper lying strata, the states of consciousness depend upon and receive their color from all the other states, and therefore every individual loves and hates in his own way, thus revealing his whole personality. Language, overlooking this difference, uses the same word for all individuals. It results, therefore, that only the impersonal elements of emotions are expressed in language. We cannot translate our impressions into words. Language is inadequate to thought. But, as a matter of fact, mental phenomena are not fixed; they vary continually; and, if it seems that they do not change, it is because we do not attend to them, but to their external causes, or, rather, to the names of the objects. Language predisposes us to errors of judgment in the matter of sensations. For example, any one eating a piece of meat which is considered a dainty can be induced to believe that it appears so to him, although it does not. In sum, the word merely expresses what is common to states of consciousness, without taking account of subjective variations. Our emotions, especially, are phenomena that change continually and in which every element receives its color from the combination in which it happens to be. When we analyze an emotion we destroy that which constituted its real and concrete individuality. We have no more than the shadows of ourselves. So that instead of having decomposed into its true components the emotion we set out to analyze, we have split it up into hard impersonal elements to which we give a name and profess to be able to apply logical laws. Thus, consciousness presents a different aspect, according as it is regarded in itself or spatially; just because the fundamental phenomena of mind are qualities that so intermingle that it is impossible to distinguish them or even to ask whether they are one or many. Mental phenomena have a manifold duration which is, as Bergeon would rightly say, a qualitative not a quantitative duration.

Now, if one accepts De Sarlo's way of stating the psychological problem, one avoids all confusion between space and

time, between simultaneity and succession, between quantity and quality. A sensation, a perception, an image, a word thought or even pronounced, a feeling, an impulse, present themselves as facts that have the characteristic of being qualitatively different and irreducible one to another. The connections which we predicate of them are different from and irreducible to one another; the connections, for example, which we posit between a sensation and a feeling are founded solely on experience, on the habit of seeing a sensation associated with a feeling, etc. The field of psychology is thus clearly limited. One may say that it embraces all that we know in the world of qualitative difference. Having defined psychology thus, it would seem that De Sarlo must accept, without change, the opinion of Wundt that psychology has the same subject matter as that of the natural sciences: since, if psychology studies the mental qualities and if all that we know in the world is represented by all these qualitative states, we ought to say that the psychological point of view is co-extensive with the whole realm of reality; thus psychology and the natural sciences would have the same reality as an object, but each would regard it from a different point of view. But, according to De Sarlo, the difference between psychology and the other natural sciences is very much greater than that stated by Wundt, Höffding and Masci. Psychology studies the origin of the various qualities in consciousness, but it does not consider them as constituting external reality; it distinguishes between objects that are conceived to be outside of consciousness, and the qualitative states themselves, that arise in consciousness. By proposing to study mental facts, we are forced to say that there are external objects which, by acting on consciousness, arouse qualitative states; but we do not mean by this, says De Sarlo, to identify mental facts with external reality. That may be demonstrated in epistemology; but from the psychological point of view we are constrained to make a sharp distinction between states of consciousness and external reality, to set the world of mind over against the external world of reality. Progress in psychology is impossible without presupposing some facts outside of consciousness. These external facts are conceived by modern science as simple quantitative variations; all phenomena are reduced to mechanical laws. One may say that every advance in physics means the possibility of reducing a determinate group of phenomena, previously irreducible, to phenomena of mechanics; the vaunted unity of the physical forces consists just in the reduction of every form of energy to particular kinds of motions. Now, De Sarlo points out that when one speaks of quantity, one means a quantity of a given quality; that these qualities for physics are atoms and motion;

but that, inasmuch as quality is uniform through all phenomena, therefore, objectively, it loses its value and the essential matter comes to be the quantitative variations. The psychological problem arises out of the need of explaining the production of mental phenomena, having given the two elements, external reality and the active subject. The task of psychology, therefore, is to see how those qualities are produced, with which we invest the external world; and, especially, the study other mental phenomena (images, memories, etc.) which we do not attribute to external reality. It is not necessary, in fact, that qualitative states should be produced by physiological conditions, in the way that sensation presupposes the action of external stimuli. In that case, we have the external stimulus that arouses the sensation; then this sensation, in turn, is a stimulus that sets up mental facts of a higher order. Just as the ovum, after it is fertilized, at first appears a homogeneous mass of cells which, at length, become differentiated into folds and layers, from which tissues, organs and systems are developed, so the mental life of sensations evolves into still more complex mental qualities. In consciousness, therefore, we see different qualities; we may say, then, that the function of consciousness is to qualify, to translate into quality, that which is presented to it. For example, a visual sensation, given under certain circumstances, forms complexes with muscular and articular sensations, thus arousing other qualities which we interpret as objective, spatial determinations, that is, as something that is not given immediately to consciousness, but, on the contrary, is already outside of consciousness. Now, one who is not acquainted with psychology thinks that the eye immediately perceives spatial relations. Psychology, however, shows that this belief is erroneous. The eyes merely convey to consciousness the local signs, sensory qualities, which have a character of their own, by which they set up complex qualities of a higher order. Therefore, psychology, as a science, comprehends a system of laws which represent the connection between determinate conditions and the arousal of qualitative states.

After making the distinction between psychology and the natural sciences, De Sarlo holds a course remote both from the old metaphysic and from naturalism. He insists that psychical causality is a causality *sui generis;* not merely irreducible to, but totally different from, the causality of natural phenomena.

Psychology, according to De Sarlo, is an abbreviated expression standing for a group of special psychological sciences, which have special characters and which are subject to the limitation of point of view and method in the light of which, and by which, the mental phenomena are studied. The chief psycho-

logical sciences are the following: a. physiological psychology; b. experimental psychology, properly called psychophysics; c. empirical psychology; d. philosophical psychology. Physiological psychology has the following main tasks. First of all, it has to show in what way the relation between the external world and consciousness is established; in other words, it studies the structure and function of the sense organs. It has, further, to show in what way the phenomenon called motion produces effects in our consciousness and how it is that motions and vibrations of the ether are transformed into excitations of nerves, before reaching the central nervous system. It studies especially the general conditions of the activity of the nervous system, in order to show why changes in consciousness are co-ordinated with changes in the body. The nervous system is related to consciousness; therefore, to examine respiratory, chemical and metabolic relations, is to study the activity of the nervous system in relation to mental activity. We have, thus, a method for studying many important mental phenomena, as sleep, waking, health, fatigue, etc. Another problem for physiological psychology is the study of the bodily effects of mental phenomena. For example, we know that emotions have physical effects which, by acting as stimuli, may give rise to new mental facts. The emotion and the bodily response thus constitute a complete circuit which physiological psychology must resolve into its elements. Finally, since we know that the nervous system and mental activity stand in a relation of concomitance, we ought, therefore, to have a chemical physiology, by which to study the cerebral localization thus given. But with this, the problems of physiological psychology, according to De Sarlo, come to an end. As a result we stand merely in the court of the temple. Whoever wishes to know the human mind must consider physiological psychology as an introduction to it. It requires special study of certain parts of physiology (physiology of the nervous system, sense organs and their connections) and it makes use of such methods only as are used in the physiological laboratory. From what has been said, the importance of the mode of envisaging psychology, according to De Sarlo, is evident. Physiological psychology does not investigate mental facts; it studies merely the concomitants and antecedents of the physical facts relatively to the mental phenomena. At the same time, De Sarlo does not make a distinction between psychology, properly called, and physiological psychology, in the sense that to the former alone belongs the interpretation of conscious experience and to the latter the derivation of the same experience from physiological processes. De Sarlo rejects such distinctions as inconsistent. There is only one method of causal psychologi-

cal explanation, and that consists in a derivation of the more complex mental processes from the more simple processes. Understood in this way, the physiological elements can always come in, by virtue of the above mentioned relation between natural experience and psychological experience, but they are always subsidiary. Materialistic psychology, in denying the existence of psychical causality, has, in place of the task set by De Sarlo, the other task of deriving psychical processes from the physiology of the brain, thus occupying a position which, as was said, is indefensible both theoretically and psychologically.

The problems of experimental psychology, according to De Sarlo, are the following. 1. To study the formation of complex perceptions, by analyzing the images and sensations that compose them; and by showing how the elements go together. 2. To investigate the time required for certain mental facts and to show how they are related to the rest of consciousness (psychometry). 3. To determine the changes of mental phenomena in relation to stimuli; also to determine the dependence of intensity of mental facts upon the intensity of stimuli (psychophysics). In all of these researches, the laboratory of experimental psychology, such as was recently established in the Institute of Higher Studies of Florence, will be indispensable. De Sarlo, who was heart and soul the sponsor of this modern foundation, has gathered about him a good number ot younger students[1] who, while pursuing various interests in philosophy, under the guidance of their teacher, enable Italy to make its contribution to the progress of experimental psychology.

We congratulate them sincerely upon this opportunity: because if there is, indeed, a line of division between the old psychology and the new, represented by the work of the German philosopher and psychologist, Herbart; if the founders of modern psychology were Weber, Fechner, Lotze, Helmholtz and Wundt; and if, from 1879 to the present, laboratories of psychology have spread and multiplied in Germany, England, France and America; Italy cannot and must not lag too far behind the other nations. In the historical development of the various

[1] De Sarlo has held a course of lectures on auditory sensations, supplementing the instruction by experiments. The results of the researches, undertaken in the laboratory, will be published in the appropriate journals. The titles are: *Automatic Movements and Mind-reading: Memory; The Perception of Time; Emotive Antagonism; Optical Illusions; Involuntary Movements in Connection with Pleasant and Unpleasant Sensations; Elementary Æsthetic Feelings; Affective Memory; A Study of Dreams; Attention and Physiological Phenomena.*

methods of scientific investigation, and especially of the experimental method, Italy too, has, glorious traditions!

Physiological psychology and experimental psychology may be considered, says De Sarlo, as two chapters of empirical psychology. In fact, in a general sense, empirical psychology embraces the study of the whole of mind, without the limitations of the two sciences just mentioned. But it has well marked characteristics which distinguish it from rational or speculative psychology. Rational psychology attempts to deduce the whole of mental life from determinate principles. Empirical psychology arises out of the need for a science of mind, the facts of which shall be arrived at, not deductively, but by means of introspection. Empirical psychology, thus, has the task, especially, of analyzing mental facts, of describing and making clear their composition and relations, by means of internal observation or introspection, and by taking account of suggestions that come from other sciences, as from psychiatry, which, dealing as it does with the complex phenomena in their disaggregation, presents an analysis naturally produced. Psychological explanation, says De Sarlo, is not identical with the explanation of natural science. Empirical psychology can establish connections between mental phenomena without knowing the ground for the connection: we can have psychological laws in the sense that we are able to follow out empirical co-existences and sequences in the qualitative states; but we are not able to study the formation of these states.

A necessary complement to empirical psychology is a metaphysics of mind, in the same way as there is a metaphysics of nature. Introspection reveals to us merely a succession of qualities; but when we wish to trace out the mechanism by which mental evolution is accomplished, we must refer to the purposes which consciousness reunites. The two edifices which the human consciousness must construct are knowledge and morality. We may say, therefore, that mental development is possible, in so far as it is directed toward the attainment of these two high ends. The metaphysics of mind cannot treat the human consciousness merely as one thing among other things, but rather as a microcosm; because, in exercising the two functions of knowledge and morality, it reflects in itself the whole universe. Considered from this standpoint, the science of mind is essentially philosophical.

A SKETCH OF THE HISTORY OF REFLEX ACTION IN THE LATTER HALF OF THE NINETEENTH CENTURY.[1]

By ROBERT H. GAULT, Late Fellow in Clark University.

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I

A STATEMENT OF THE THEORY AND OF THE EXTENT OF KNOWLEDGE WITH REGARD TO REFLEXES AT THE TIME OF THE LOTZE-PFLÜGER DISCUSSION (1853.)

Before making in greater detail the statement which is the subject of this section it seems well, in order to bring the reader into touch with the development of knowledge of reflexes previous to 1853, to give in brief form a running account of the comprehensive article by Doctors G. S. Hall and C. F. Hodge in the *American Journal of Psychology*, Vol. III. These papers present a sketch of the history of Reflex Action from the earliest studies thereof to the time when Lotze made his famous reply to Pflüger and formulated the theory of reflexes which bears his name.

It is there shown how, at first, the soul was supposed to be able, without the mediation of any bodily mechanism whatever, to create sympathy between different parts of the body. The type of mind of the early physiologists was theoretical rather than scientific. Indeed, so long as those early investigators were explaining bodily functions of every character by reference to an immaterial soul inhabiting the natural channels of the body, and acting directly upon matter according to its

[1] The writer wishes to express his obligation to Dr. E. C. Sanford, on whose suggestion this work was undertaken and by whose counsel it was carried to completion.

own freedom, no scientific physiology was possible. But as time advanced new phenomena were discovered which could not be classified under existing theories, and the new hypotheses to which they gave rise only increased the confusion.

Harvey discovered in 1628 that the arteries and veins during life are filled with flowing blood. The supposition that these channels, empty after death, were during life the avenues for animal spirits had to be abandoned; and Robert Whytt advanced the matter to a new stage by his explanation of the phenomena of life by the "power or influence of the nerves." The current was setting toward the Newtonian ideal, the maintenance of steadfast communication with facts.

Very early it had been found that some nerves when stimulated mediate sensation, others motion. It was left, however, for Charles Bell to show, and for Magendie more conclusively to demonstrate, that the posterior spinal roots are sensory, and the anterior motor. The way was then opened for further classification of phenomena. The anatomical elements concerned in reflex action, viz.: a centripetal and a centrifugal nerve with their part of the spinal cord, were known, and the next question was, "do these parts operate on mechanical principles or not?"

The first reply to this great question was in the affimative. Marshall Hall (1837) first proposed a mechanical theory of reflex action. He assumed a special excito-motor system in the cord and said: "There is nothing whatever that can be called psychic about any of its activities." He was enthusiastically supported by his German translator, Kürschner, who said that the activity of ganglia incites motion "as water drives a mill." Volkmann (1838), on the other hand, declares that in the lower animals, at least, it is certain that the sensitive principle is divisible; that, therefore, whenever the central nervous system be divided a portion of this principle remains on both sides of the section. Such animals cannot then be pure mechanisms as Marshall Hall had supposed. Volkmann, however, is not sure that this principle is valid for the higher animals.

Pflüger was bolder. Following in the footsteps of Volkmann, he violently asserts that consciousness (which he regarded as a form of motion) is, whether we call it "Sensorium" or "Soul," capable of division with the body. The spinal cord, then, has its soul; every portion of the body which, independently, is capable of responding to a stimulus, possesses a soul. "Reflex action is the operation of a neurophysic mechanism by means of which the sensory fibre through the cord, changes the ordinary state of excitation of definite motor nerves."

This brings us to Hermann Lotze and to the beginning of that period which furnishes the material for the following sections. In the Lotzean theory the soul has nothing to do directly in the production of reflex actions. Under certain limitations the theory may be described as mechanical; but all movement, whether of the intact animal or the reflex preparation, is proximately or remotely subject to the psychical. The starting point of all movement is in the soul. "If we call to mind, consequently, that activities earlier performed in consciousness have left behind not only unconscious memories in the soul, but also physical impressions in the central parts of the nervous system, we can consider purposive and adaptive movements to be dependent upon these impressions as well as upon a soul still in existence. For even the soul itself in the production of these movements should be considered not as an intelligent substance but as a substance with enduring states standing in reciprocal relation with one another and reproducing one another."

"The origin of certain movements of beheaded animals we seek not in an intelligence yet living, but in one which exists only in its after-effects. We believe that an animal body whose soul has had no experiences, or which has not worked out some experience into a life of ideas, would not be in a condition to perform those movements after the excision of its brain; we consider it not as a mechanism of the first construction, but as one of practice. When, under the influence of the soul life an association has once been formed between the mere physical impression of a stimulus and a movement which is not united with that stimulus by the mere relation of structure and function, and when that association has been firmly established, this mechanism can continue the activity without requiring the actual assistance of intelligence."

"In animal bodies examples of such habituation in function to which the influence of mental life does not extend are by no means wanting. Yet, much more frequently, we find them in the sphere of movements. Not only are almost all reflex movements produced the more easily and by slighter stimuli the more frequently they occur, but even the voluntary movements through practice become graceful and manageable. Many gestures, originally arising by chance, have gradually become customary, and we find them firmly and ineradicably rooted; finally the acquired mien, movement, and grace of the body is frequently transferred from generation to generation, which could hardly occur if the repeated function were not fixed in a remaining disposition of the central organs, and hence, were not capable of transplantation. As stature has placed at the disposal of the soul a principality of automatic

implements, the activity of the mind reacts upon it, giving it dignity, and the body is impregnated by the attainment of an intelligence which, nevertheless, is not identical with it, is not diffused through it, and at the same time is not separable from it."[1]

According to this theory the movements of the eel's tail away from the flame, and the purposive movements of the decapitated frog, cited by Pflüger as indications of a spinal soul, are not due to the presence of a soul in the cord, but to the after-effects of conscious activity impressed upon a plastic organization and transmitted by heredity. Lotze had, therefore, caught the idea of development from the conscious to the unconscious; the voluntary to the involuntary; from the spontaneous to the reflex; and we shall hope to see in the succeeding sections how this theory has stood the test of fifty years.

We shall now turn to a brief examination of the state of experimental knowledge of the physiology of reflex action at the time in question.

Among the most numerous observations are those by Pflüger, and since they suggested so many lines of research for the investigators who were to follow, his experiments are of great importance. He believed that reflex actions are best studied in men, especially in human pathological cases, and on the results of such studies he based his five laws of reflex action: the law of Unilateral Reflexes; of Reflex Symmetry; of Unequal Contraction on the Two Sides; of Reflex Irradiation; of the three Locations of Reflex Contraction. These laws were illustrated by many experiments on frogs, eels, and salamanders, and from these he concludes that sensory and motor nerves end in their respective levels of the cord, not in the brain which many regarded as the only organ of sensation and volition. As might be expected from a study of the Lotzean theory, its author declares that Pflüger's crucial experiments are too uncertain to warrant the inferences drawn from them.

It seems measurably correct to visualize the course of the studies of reflex action preceding the Lotze-Pflüger discussion as lying along the main trunk line of "physical" reflexes as they were then called; *i. e.*, bodily movements of defense against attacking stimuli. Nevertheless there had been some minor observations of the movements of the internal organs which were called "organic" reflexes, and which opened the way for later thorough investigations made possible by the discovery of improved devices for experimental observation, and improved methods of operation.

What has now become an almost impenetrable jungle of

[1] Lotze: Gelehrte Anzeiger, Göttingen, 1853, III, pp. 1737 ff.

literature on the inhibition of reflex action sprang from seeds sown by Marshall Hall, though the phenomenon was not subjected to detailed examination until a quarter of a century had passed. The observation made by Hall is now a commonplace with every amateur experimenter with reflex actions, viz.: that reflex movements cannot be produced in the first moment after decapitation. Whether this is due only to the shock to the central nervous system, as is now very generally believed, or to stimuli from the cut surfaces, is left for future determination. A typical instance of the restraining influence of nervous centres upon a particular bodily organ is the inhibitory effect of the vagus upon the heart. This was discovered by the Webers in 1844,[1] and almost forty years later McKendrick called it the "most recent advance in *nervous physiology*."[2] Our present knowledge of inhibitory phenomena, in detail, was, however, in 1853, scarcely begun.

Physiologists were not introduced to the summation of stimuli until the year following the discussion between Lotze and Pflüger, when Eduard Weber computed the rate of transmission in sensory nerves; and no really systematic work was done on the subject until nearly a score of years later.

Neither had the knowledge of vascular tonus made much further progress than that of inhibition, though Legallois, as early as 1830 or before, had observed that the circulation of the blood ceases much more quickly than otherwise if the spinal cord be destroyed after decapitation. And changes in the lumen of blood vessels had been observed from time to time. Indeed, it may be doubted whether, within the period of which we are now writing, at any rate until its very close, the tonicity of the vascular system was ever regarded as reflex. We have such eminent authority as J. Lister and Gruenhagen to support the statement that a notion of the reflex nature of the tonus of the vascular system begins with Claude Bernard in 1852 and Augustus Waller in 1853. For Bernard in the year named had shown that if the sympathetic nerve in the neck of a cat be severed the blood vessels on the wounded side become turgid, and the temperature of the parts is heightened. Waller, in the year following, completed the demonstration by showing that if the nerve above the point of section be stimulated by an electric shock, turgescence ceases and temperature subsides.

This bit of experimental knowledge forms an indispensable nucleus about which later scientists have built a vastly complicated structure. The cause of a reflex muscular tonus, on the other hand, is within this period much less clearly conceived.

[1] Artikel—Muskelbewegungen—Wagner's Handwörterbuch d. Phys. Bd., III, Abth 2 p. 47, 1844.

[2] McKendrick: A Lecture on Physiological Discovery. *Brit. Med. Jour.*, 1883, I, p. 708.

The term "tonus" was first used by J. Müller[1] to describe the tense condition of muscles in a state of rest, and Henle assumed that it is to this slight tension of the muscles of the face that we owe our natural facial expression. This tension was supposed to be dependent upon the constant influence of the central nervous system, and Hall and Volkmann shared the view. Although Ed. Weber, in 1844, revolted against the theory of such a muscular tonicity, it was the prevailing view held at the end of the period covered by the present chapter, and the strong reaction against it led by Auerbach and Heidenhain, distinctly marks the beginning of the following period of the study of muscular phenomena.

The tendon reflexes did not attract the attention of physiologists until nearly a quarter of a century after the beginning of the period we are reviewing.

If we except, then, the discovery of the inhibitory effect of the vagus upon the heart by the Webers, we see that the chief results obtained previous to 1853 were in the field of the bodily or limb reflexes which led to the formulation of Pflüger's laws. Thereafter, as a result of more delicate means of observation, increasing attention was given to the uniform and varied movements of the vital organs. The notion of the reflex character of organic behavior steadily encroached upon the field of the unknown. The reflex arc took the place of many a supposed spontaneous activity, and investigators since that time have gradually left the highways of philosophy for the intricate byways of physiological experiment. They have become more interested in the collection of facts than in their correlation; and more and more dominated by a desire for "pure" science. This is very likely the spirit in which all great achievements have been won, but it is likewise the spirit which leads to the production of a mountain of details, inexhaustibly rich in definite suggestions to other investigators and bewildering to those who attempt to set them in order.

So few within this period are the generalizations and discoveries of general importance to the comprehension of reflex action that it is difficult to make clear cross sections of the whole and to treat all between in a single chapter. We may, with considerable justification, consider the beginning of our fifty years as the point in the main current of our knowledge of reflexes where the stream breaks up into many parallel branches.

We shall, therefore, for the sake of convenience, treat these separately in their physiological groupings.

[1] J. Müller: Handbuch d. Physiol., 1837, Bd. II, p. 29.

II.

The Inhibition of Reflexes.

The problem of reflex inhibition is everywhere veiled in uncertainty. Consensus of opinion on this subject seems like a "Will o' the Wisp;" now almost at hand, the next moment far out of reach. And to-day our knowledge of the matter is hardly in a more settled state than it was a score and more of years ago.

This uncertainty is due, doubtless, to the difficulties that lie in the way of experimentation. Goltz, for instance, has shown[1] that after an operation has been performed, and the wound has healed, we have to deal with the vicarious functions of other parts. Hence we can never tell just what are the inhibitory functions of the injured part. Luciani has also called attention to the fact that we can never be sure how soon after the operation degenerations have begun in the cord nor how rapidly they have extended,

Following the discovery by the Webers of the influence of the vagus upon the heart, physiologists turned their attention especially to the function of this nerve. Schiff and Moleschott erroneously concluded from numerous experiments that it is motor to the heart. Pflüger, however, in his characteristically vigorous, not to say pugnacious, way demonstrated beyond a doubt that the heart is inhibited by the vagus nerve.[2] If the simplest rules of experimental practice in the application of electrical stimuli be employed, *i. e.*, if the vagus be isolated, no motor effect is discernible. On the contrary, as he says by way of conclusion: "The stimulation that retards the frequency of the pulse manifests itself on the heart through no other symptom than the prolongation of the diastole. If the vagus, as Schiff and Moleschott assert, can increase the frequency of the pulse, even in the face of high exhaustibility, at least the diastole must be shortened and contraction prolonged. Instead of this only the first is seen: prolongation of diastole." Pflüger seems to have observed the phenomena very accurately, for Howell, in 1896, agrees with him in describing the effect of vagus stimulation. How the vagus inhibits, however, and how the heart is excited to activity, are matters less easily decided. Stewart found, in 1892, that during moments of greatest intra-cardiac pressure it is most difficult to inhibit the pulsation of the heart by stimulation of the vagus. This seems to indicate that the motor apparatus is located in the muscular walls of the organ itself. Of recent

[1] Goltz: Ueber d. Verrichtungen d. Grosshirns. *Pflüger's Arch.*, XIII, pp. 1 ff.

[2] Pflüger: Untersuch. aus d. Bonner physiolog. Lab., 1865, pp. 1 ff.

times the most widely accepted explanation of inhibition by the vagus is that which Stefani proposed in 1880, and Gaskell somewhat later. The vagus is the trophic nerve of the heart. It is katabolic in systole and anabolic in diastole. The after effect of vagus excitation, says Gaskell[1] somewhat later, is to strengthen the force of the cardiac contraction and to increase the speed with which excitation waves pass over the heart. The contrary effects on the other hand appear on stimulation of the augmentor. This theory of vagus inhibition as due to trophic effects is not without positive experimental evidence, for Fantino, in the same year in which Gaskell wrote his defense of the theory, cut the left vagus in a rabbit and reported the changes which he observed in the heart wall. The animal was apparently healthy, ate well, and its weight increased. But on examination eighteen days after the operation it was found that the heart had atrophied to a considerable extent. The region over which the waste of tissue occurs, moreover, differs according as the right or left vagus is injured.

There are but three theories which have at any time received wide acceptance.[2] None of these can be sharply limited chronologically or by generally important physiological discoveries. They are tenacious of life and each overlaps the period during which the succeeding theory attained its prime.

Setschenow's Centre Theory. The first is known as Setschenow's centre theory.[3] Setschenow discovered that while stimulating the optic lobes and mid-brain of a frog reflex movements of the limbs could not be induced by peripheral stimulation as under ordinary circumstances. These parts he calls the centre for inhibition. They are kept in tone by continuous sensory impressions. This theory has enjoyed a long life. Even to-day some eminent physiologists have not positively decided against it. Tigerstedt, for instance, in 1897, gives it as his opinion, that it must be considered as undecided whether inhibitions can be explained by the centre theory.[4]

Setschenow was at first inclined to the opinion that the

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[1] Gaskell: *Arch. de Physiol.*, 1888, I, pp. 56 ff.

[2] Of the explanations offered for particular cases of inhibition we cannot now speak, but must pass on to trace the development of the theories of inhibition in general. One of the earliest theories was offered by Schiff who believed that inhibition was merely a case of exhaustion. Pflüger held a theory of special inhibitory ganglia in the heart and intestinal walls and elsewhere, a theory which was more satisfactory at the time. There were also other minor theories, but none have more than an antiquarian interest at the present time.

[3] Setschenow: Phys. Studien über d. Hemmungs-Mech. f. d. Reflexthätigkeit d. Rückenmarks d. Frosches, 1863, pp. 1-71.

[4] Tigerstedt: Physiol. d. Menschen, Bd. II, 1897, p. 285.

inhibitory centres lie only in those parts of the brain which have been mentioned, because direct stimulation of the surface of a transverse section of the cord has not the same effect as stimulation of the optic thalami. This, however, was later shown to be insufficient, and when Herzen and Schiff showed that, in a completely debrained frog, stimulation of the posterior parts inhibits the movements of the fore limbs, Setschenow extended his theory and declared that inhibitory centres exist also in the spinal cord.

Preyer supported the theory of inhibitory centres on the ground of his observations of the phenomenon of kataplexy[1] and explained hypnosis by inhibition. Of the inability to produce violent movement during the kataplektic state, he says: "The excitation of the reflex inhibitory apparatus outweighs that of the psychomotor centres." Nothnagel in his argument for the centre theory even went so far as to ascribe to the inhibitory centres the power of voluntary activity.

It had been explained that there were definite inhibitory centres in the brain and spinal cord, and the followers of Setschenow attempted to discover the avenues through which excitations from the periphery reach these centres. Setschenow himself had considered the eye as the chief medium for this purpose, but Langendorff, on repeating Goltz's experiments on the reflex croak, and finding that by pinching the frog's toe the croak could be inhibited, concluded[2] that the skin was another avenue for these excitations. Inhibition was, he thought, purely reflex.

But, as Schlösser pointed out, if one grants all that the experiments seem to show there is no indication as to how these centres can act,[3] and further, in order to account for the numerous inhibitions actually found, the centres must be exceedingly numerous or must form a very complex system. Goltz comments upon this same difficulty as follows:[4] "Many have undertaken to assume (for the explanation of numerous experiments, which may be made clear much more simply in another way) inhibitory centres in the heart. These should be inhibitory centres of the first order. The heart is brought to a standstill by the mediation of the vagus nerve from the medulla oblongata; therefore in the medulla there would be an inhibitory centre for the heart which should be the inhibitory

[1] Preyer: Die Kataplexie u. d. thierische Hypnotismus, Sammlung, phys. Abhandlungen, Jena, 1875, Secs. 17-42.
[2] Langendorff: *Du Bois Reymond's Arch.*, 1877, 4 u. 5 Heft, pp. 96-115 and 435-442.
[3] Schlösser: Untersuch. über die Hemmung von Reflexen. *Du Bois Reymond's Arch.*, 1880, pp. 303-322.
[4] Goltz: Nervencentren d. Frosches, 1869, p. 50.

centre of the second order. This inhibitory centre can, however, as I have shown, be inhibited if the skin of the extremities is strongly stimulated. For this we need an inhibitory centre of the third order to bring the centre of the second order into a state of inactivity. . . . ."

Moreover, there are anatomical difficulties in the way of the full acceptation of such a theory as Setschenow's. It is a plausible theory that if there are specific centres for all inhibition there must also be special inhibitory nerves for voluntary muscles as well as for the heart and intestines. The question has been amply discussed both pro and con. Wundt tells us[1] that there are inhibitory nerves leading to muscles and to glands. They are most probably connected with the muscles by reflex inhibitory ganglia. But Amaya points out[2] that while it may be assumed that inhibitory fibres are contained in the nerve trunks they cannot be isolated from the motor fibres by any process of degeneration. A still further difficulty with the centre theory, which Setschenow himself admitted, and which shows its incompleteness, is that by it the inhibition of the tactile reflexes cannot be explained since for these Setschenow failed to find any specific centres.

The Theory of Goltz. The unreasonableness of having to explain the inhibitions of different reflexes by different theories led Goltz to offer a new explanation. This was more theoretical than the former, but its author hoped it would cover all cases. He proceeds from the inhibition of the croak in the frog, which he calls a good type of all inhibitions. The hypothesis rests upon the supposition that "a centre which mediates a definite reflex act loses excitability for this act if it is set in excitation at the same time by any other nerve tracts which are not concerned in that reflex act." The inhibition of tactual or other reflexes, says Goltz, can be explained on this theory, and this constitutes its great advantage. If a reflex frog's flank be acidized, the spot is wiped by the foot of the adjacent side, whereas the normal frog leaps away. From the ever active brain, he says, excitations are always flowing to every centre; and thus, it comes about that in the normal frog the movement to wipe off the acid is suppressed, and the movement of escape is substituted for it.

Wundt also discusses the influence of the higher centres upon the occurrence of reflexes.[3] "This point of view," he says, "casts light upon the slighter intensity of reflexes which one generally observes so long as the brain is retained. In this

[1] Wundt: Lehrb. d. Physiol., 1878, p. 528.

[2] Amaya: Ueber scheinbare Hemmungen aus Nervmuskelpräparate, I u. II, 9te u. 10te H., pp. 413 ff. 425 ff.

[3] Wundt: Mechanik d. Nerven, 2te abth., 1876, p. 100.

case simultaneously with the stimulation of the reflex organ occurs also a stimulation of the brain in which conscious sensation arises. The sensation is probably only an accompanying phenomenon and stands in no direct relation to inhibition. But according to the rule that every interfering stimulation of the central spheres in which centripetal fibres end inhibits reflexes, so in this case also, through the interference of spatially separated stimulus-effects of one and the same stimulation, an inhibition can occur."

This explanation, says Goltz, has in it many points of similarity with the theory previously offered by Herzen and Schiff, viz.: that inhibitions are due to the exhaustion of nerve centres. Herzen had said that any strong sensory excitation removes the reflex excitability of the nerve centres because they are exhausted by the intensity of the excitement. On the other hand Herzen's contention that reflexes are heightened by the debraining operation because thereafter the sensory excitations have a narrower sphere in which to be effective, is not approved by Goltz for a reason similar to that offered by Wundt, namely, that inhibitions are more intense the better the condition of the animal for the transmission of stimuli.[1] There are numerous phenomena to support this objection.

Taken as a whole the literature contains fewer direct references either of approval or condemnation of this view than that of Setschenow, but there are many who lend it their support by implication. Among these is Wundt, who thinks that when two or more excitations reach a sensory centre their effect is inhibition.[2] Howell, too,[3] says that to obtain inhibition, there must be at least two pathways by which impulses may reach a cell, and the stimuli conveyed over these different tracts must tend to excite different reactions. Inhibition, therefore, is connected with the effects of sets of impulses upon a responding cell, and this is associated with the fact that as the two paths end in different relations to the cell the impulses must enter it at different points, and hence, tend to act on different portions of the cell contents. There is enough known, he says, to conclude that inhibition does not depend upon special fibres but upon several impulses coming in by different paths. Schlösser, however, is one of those who opposes this theory.[4] He is convinced that all the phenomena of inhibition can be explained on the supposition of antagonistic centres, and is unable to find any proof for the disturbance of an excitation within the cell. In fact, such proof, it may be said in passing,

[1] Wundt: Physiol. Psychol., 5 te Aufl., Bd. I, 1902, p. 86.
[2] *Op. cit.*, Bd. I, p. 87.
[3] Howell: Am. Text Book of Phys., 1896, p. 667.
[4] Schlösser: *Op. cit.*, pp. 303-322.

cannot be experimental, and Goltz himself did not expect to find it. Tigerstedt leaves the question open. Verworn approaches the matter in an attempt to answer the question: "What relation exists between a centre and a muscle when a muscular contraction is inhibited?"[1] He was able to disprove the assertion of Starke that in inhibition of skeletal muscles the excitability of motor roots is reduced. On the other hand he says this excitability is unchanged. Hence there is no active inhibitory impulse opposed qualitatively to a motor impulse conducted to a muscle through its nerves. In other words: "The process of inhibition arising in the ganglion cell bodies of the anterior horns is not extended to the axis cylinder of the neurones, and is not transmitted through them to the skeletal muscles, nor transmitted actively in any way." He further concludes that skeletal muscles also possess no particular inhibitory fibres; that all inhibitions which occur in the skeletal muscles, *e. g.*, reflex inhibitions, are purely passive, and occur only in a central way through inhibitions of the central elements, and consequently consist only in a simple cessation or dropping out of the central excitatory impulses to the muscles. On the other hand he does not deny that active inhibitions are present whenever muscles have a particular autonomy or a particular tonus, as is the case with the muscles of the heart, blood vessels, and intestines. Here, indeed, there are special inhibitory nerves whose excitation causes an inhibition of the end organs in the muscles.

We have now seen some of the difficulties in the way of the centre theory, and also the theoretical character of the explanation offered by Goltz. It remains to speak of another explanation no less theoretical than the latter, but sharing with it a very generous share of popular approval.

The Theory of Lauder Brunton. To say that all inhibitions are due to the fact that the central nervous system does not permit its different parts to be simultaneously excited by different stimuli, leaves us in uncertainty as to how the nervous system comes to have such a property. An interference in the nerves themselves has accordingly been suggested. The first great exponent of this theory was Lauder Brunton. In its earlier stages his explanation was about as follows.[2] The excitation following a weak stimulation is carried only to a motor cell and produces movement. A strong stimulation passes over to an inhibitory cell whose activity cancels that of the motor cell, wholly or in part. This, however, did not suggest the doc-

[1] Verworn: Zur Phys. d. nervösen Hemmungserscheinungen *Arch. f. Anat. u. Phys.*, pp. 105 ff. Suppl. Abth., 1900.

[2] Brunton: West Riding Asylum Reps., 1874, pp. 179-222. See *Nature*, Vol. 27, p. 437.

trine of interference, because Munk had shown that in different nerves the rates of transmission are different, and interference cannot be used as an explanation unless the rates of transmission are the same. Later it came to be believed that under like conditions the rates are similar, and Romanes took a forward step in experimental physiology[1] which led Brunton shortly afterward to state the theory we are about to discuss.

Romanes' experiments consist in tests on a strip of Medusa tissue at one end of which is a single lithocyst. He first observed the rhythm of activity of this lithocyst and then at the same rate stimulated the other end of the strip by electricity. The waves then always passed from the stimulated point toward the lithocyst. But if the rate of stimulation fell below that of the lithocyst, after one to six waves, depending upon the degree of conformity thereto, one passed in the opposite direction. When two such waves met they neutralized each other; or the tissue on each side of the meeting point, having lately been excited, could not transmit another excitation. The facts of inhibition then are explained as due to an interference of vibrations the rates of which are not synchronous.

Brunton now formulates the theory of interference on the analogy of the interference of waves of light and sound.[2] "Motion, sensation, inhibition, or stimulation are not positive but simply relative terms, and stimulating or inhibiting functions may be exercised by the same cell according to the relation which subsists between the wave lengths of the impulses travelling to or from it, the distance over which they travel, and the rapidity with which they are propagated."

The test of this theory is to be found, according to Brunton, in those variations of the conditions of temperature, etc., which change the rate of transmission in the nerves.

The effect of warmth or cold on strychnine tetanus, says Brunton, is what we should expect on the theory of interference. If small doses of strychnine are given warmth abolishes the convulsions but cold increases them; the contrary follows when large doses are given. The explanation he gives is in detail as follows: "If a small dose of strychnine retard the transmission of nervous impulses so that the inhibitory wave is allowed to fall more than half a wave length, but not a whole wave length behind the stimulus wave, we should have a cer-

[1] Romanes: On the Locomotor Systems of Medusæ, Philosoph. Trans., 1877, pp. 659-752. See Gen'l Summary, pp. 745 ff.

[2] Lauder Brunton: On the Nature of Inhibition and the Action of Drugs upon it. *Nature*, 1883, Vol. XXVII, p. 422. See the series of articles under this subject; same volume, pp. 419 ff., 436 ff., 467 ff., 485 ff.

tain amount of stimulation instead of inhibition. Slight warmth by quickening the transmission of impulses should counteract this effect and remove the effect of strychnine. Cold, on the other hand, by causing further retardation should increase the effect. With a large dose of strychnine, the transmission of the inhibitory wave being still further retarded, the warmth would be sufficient to make the two waves coincide while the cold would throw back the inhibitory wave a whole wave length, and thus again, abolish the convulsions."

Several other facts are offered by Brunton in favor of his theory. There is the well known phenomenon of accelerated heart activity during the act of swallowing because a part of the stimulus of the bolus, in the pharynx, passes to the centre in the medulla oblongata which is close to the root of the vagus. This nerve itself is inhibited and the heart works more rapidly. Stimulation of the sciatic nerve in a frog may inhibit the reflexes on the other side. A few hours later when conditions may be supposed to have changed, instead of inhibited response we find clonic convulsions. In the frog heart, according to the description given by Beale, Brunton finds a mechanism fitted for altering the distance two stimuli have to travel and thus allowing them to interfere with and to inhibit each other, and finally there is the observation of Mortimer Granville,[1] that by rapid percussion of a nerve already mediating a grinding pain, and by slow percussion of one mediating acute pain, he is often able to cancel the discomfort.

If we could be absolutely certain of that of which Mercier seems so confident,[2] viz.: that "nerve currents are known to be undulatory in form," and if we could be sure that, as he says, and as Loeb somewhere affirms, along every nerve fibre gushes of nerve force succeed each other as waves of blood in arteries and veins, we could more easily conclude in favor of this theory of inhibition by interference. Doubtless, as Herrick says,[3] it is the most generally accepted theory. But in the face of the fact that Wundt declares that there is no evidence for it,[4] and J. Kron, after an elaborate series of investigations in Mendel's laboratory,[5] comes to the conclusion that inhibitions are due to the changes in the supply of blood to the nervous centres, it is probably safer to do as so many others have done

[1] Mortimer Granville: Nerve Vibration and Excitation. London, 1883. See *Nature*, Vol. XXVII, p. 437.
[2] Mercier: The Nervous System and the Mind, p. 74.
[3] Herrick: Baldwin's Dict. of Philos. and Psychol. Art. *Inhibition*.
[4] Wundt: Physiol. Psych., Bd. I, 1902, p. 87.
[5] J. Krou: Experimentelle Beiträge z. Lehre von d. Hemmung d. Reflexe nach halbseitiger Durchschneidung d. Rückenmarks. *Deutsche Zeitsch. f. Nervenheilkunde*, 1902, Bd. 22, Dec. Heft.

before, that is, to leave the question open for further investigation.

III.

The Phenomena of Summation.

Closely allied with the phenomena of inhibition are those of summation. If the recent prevailing views concerning these functions are correct one may almost say that they represent opposite faces of the same phenomenon. The summation of stimuli, true to the etymological significance of the phrase, is a heaping up of impulses. Every one is familiar with the extreme exasperation to which one may be driven by a fly which persists in attacks upon one's forehead, and with what vigorous gestures we seek once for all to annihilate the offender. An experience scarcely less well known is the irritation caused by a small body fast within the throat. First we cough slightly, then violently, and finally, if the offense continues, we fall into a cramp and emerge therefrom in a weakened, breathless state.

At the beginning of our half century of physiological history summated stimuli were little understood, and had not been specially examined.

Helmholtz[1] early gave a clue as to how these phenomena might be explained in calling attention to the slow rate of transmission from the sensory to the motor roots in the cord. (Exner's "reduced reflex time."[2]) He estimates that this transmission required from 1/30 to 1/10 sec., a much slower rate than that of impulses in the nerve fibres. This may be taken in connection with the early observation by Schiff on frogs, that the thinner the gray matter of the cord was made between the sensory and motor roots in the single locality tested, the longer is the reflex time.[3]

These facts suggest the crowding of impulses into a narrow passageway, or into a channel which offers some resistance, the later thus overtaking the earlier and heaping up upon them until of their own accumulated energy they break through *en masse*, pass over to the motor roots and occasion violent movement. The early methods of investigation, however, were inadequate to furnish any very reliable information regarding the time that must elapse between the application of a stimulus and its motor effect, or concerning the relations of interval and stimulus number best suited to optimum results. Setschenow showed that ability to summate stimuli belongs in an especially

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[1] Helmholtz: Ber. d. k. Akad. d. Wissensch, z. Berlin, pp. 329 ff., 1854
[2] Exner: *Pflüger's Archiv.*, 1874, Bd. VIII, pp. 526 ff.
[3] Schiff: Lehrb. d. Physiol., 1858-1859, p. 228.

high degree to the locomotor centres;[1] and that if a galvanic current be interrupted sixty times per minute, the reflex quiver occurs in a slight degree after the first interruption, and is confined to a limited number of muscles; the second, third, etc., interruptions are followed by more intensive and extensive contractions until a movement of the whole extremity follows. These experiments were with electrical stimuli. But Türck[2] much earlier and Baxt[3] somewhat later obtained the same result with chemical stimuli, and the latter set forth his law that the reflex time increases in geometrical progression while the degree of acidity of the stimulus decreases in arithmetical progression. Such a stimulus, he says, can occasion excitation only if its particles follow one another into the nerve tissue more rapidly than excitation passes away.

But the difference in the rates of transmission of different parts of the nervous system and the observation made by Schiff mentioned above did not open a way in the minds of all for the explanation. One of Schiff's own contemporaries accounted for the phenomena on the ground of the increased excitability of nervous tissue[4] and thought that this excitability can be measured by the position of the secondary coil required to produce a reflex as two, three, or four times a former excitability. Heidenhein in the same year found that muscle in a state of contraction is more excitable than at other times. Gruenhagen, however, denies that the phenomena in question are due to an increased excitability of muscular tissue or of any other tissue.[5] Pflüger had declared that during the process of death the positive forces of inhibition vanish more quickly than the forces of molecular tension. Hence when a stimulus is applied, in the absence of inhibitory forces, the nerve *appears* to have a heightened excitability. Harless, further, showed in a later work than that mentioned above that the simultaneous stimulation of two parts of the same nerve acts more strongly than a single stimulation of one part.[6] This, says Gruenhagen, is easily established.[7] The upper part of a nerve is more excitable than the lower as can be easily shown by the comparison of the effects of two electrical stimuli one passing up, the other down the nerve. No less plausible, thinks Gruenhagen, is Pflüger's

[1] Setschenow: Monog. über d. elec. u. chem. Reizung d. sensib., Rückenmarknerven d. Frosches, Gratz, 1868, I, sec. 12, u. a.
[2] Türck: *Zeitsch. d. Gesell. d. Aerzte*, 1850, H. III.
[3] Baxt: Arbeiten aus d. phys. Anstalt z. Leipzig, 1871.
[4] Harless: Ueber Muskelzuckungen bei dem Vertrocknen d. Nerven. München, Gelehr. Anz., XLVIII, 1859, col., 241-246.
[5] Gruenhagen: Bemerkungen über d. Summation v. Erregungen in d. Nervenfasern. *Zeitsch. f. rat. Med.*, pp. 190 ff. XXVI, 1866.
[6] Gruenhagen: *op. cit.*
[7] Gruenhagen: *op. cit.*

decision that in the absence of inhibitory forces nervous tissue possesses only apparently a higher degree of excitability than formerly. Gruenhagen assumes that in an animal in which the lower and higher nervous centres have been separated, the moisture contained in the nerves begins to evaporate. This process occasions more or less of a crumpling or folding of the nerve masses, and this in turn a stimulation of the fibres which, within certain limits, is the more intense the further the process of evaporation has proceeded. Any peripheral stimulus produces an excitation which is added to this aroused internally, and a more vigorous contraction attends a weak peripheral stimulus than, in a fresh state of the nerves, accompanies a much more intense external stimulus. Gruenhagen thus arrives at a conception of summation. Pflüger had shown that of two frog shanks one stimulated from the plexus, the other from the muscle itself, the former is the more quickly thrown into contraction. This, he explains, on the assumption of an avalanche-like swelling of excitations as the excitement of the plexus proceeds toward the muscle. Gruenhagen, however, prefers, in this connection, to speak of a rolling up of nerve force.

Previous to 1874 no very systematic work had been done on this subject. In that year, however, William Stirling published his work on summation[1] in which the phenomena described are discussed in detail. In none of the tables which he presents is any relation whatever to be found between the intensity of the stimuli and the time of latency. This much controverted point has been confirmed in our own times by Goldscheider. But latency does depend invariably, as Stirling shows, upon the frequency with which stimuli follow one another. Though it varies somewhat according to the part of the body as well as with the animal on which tests are made, *e. g.*, Tigerstedt shows that 27 stimuli per second produce tetanus in the gastrocnemius of the frog while 10 per second have the same effect on the red muscle soleus in the puppy. Other things equal, the shorter period of latency always accompanies the shorter stimulus interval. It is inadmissible to compare the results of several experimental series with one another even though the stimulus interval be uniform throughout for the reason that at different stages in the life of a preparation these periods vary considerably from causes which we cannot compute. For instance, with a constant interval of $\frac{1}{8}$ second variations in latency in a series of experiments may occur between .5 and 5 seconds: with an interval of $\frac{1}{4}$ second the variations

[1] Wm. Stirling: Summation v. elec., Hautreizen, Ber. d. sächs. Acad. Math. phys., 1875, pp. 372 ff.

range from 1 to 30 seconds. As the interval increases above the optimum he shows, in analogy with Baxt's law, that the number of stimuli necessary for producing a contraction must surpass by many fold those required where a shorter interval is used.

Can any reflex movement occur without a summation of stimuli? All agree that much greater intensities of currents are required for the production of reflexes by a single stimulus than in the classical summation experiments in which a series of relatively weak stimuli are used. To find the cause of this difference Stirling compared in many ways the effect of single induction shocks with that of repeated stimuli. All apparently spontaneous movements of reflex animals he was led to put down, incidentally, to the account of summation.

To the objection that demonstrably simple stimuli may produce effects such as are attributed to summation, Engelmann replies[1] that very intense induction shocks produce chemical and thermal variations in the substance of the nerves, and Stirling has observed that a sensitive frog shank, with a single shock, is often thrown into tetanus. He would, therefore, bring single stimuli into the sphere of summation, and state it as a general law that reflexes can be called out only by repeated stimuli.

The problems of summation were later carried into a field of more immediate interest to the psychologist. Recent studies furnish some evidence of a connection of summation with sensation. Naunyn in his study of the sensation of pain[2] has found that under the conditions observed in summation experiments in general the period of latency of pain summation is the same as that of the summation of sensory stimuli for the production of reflex movements. This, however, as Naunyn recognizes, does not establish the matter beyond a doubt but only makes it probable that pain is a product of summation.

Goldscheider brings the "secondary sensation" into relation with the summation of stimuli.[3] If one's skin is pricked with a needle, after a sensationless interval an after-sensation appears which, though of varying degrees of clearness, cannot be mistaken. In the course of his experiments he found that there is no definite relation between stimulus number and stimulus interval: different intervals are favorable for a clear after-sen-

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[1] Engelmann: Bewegungserscheinungen an Nervenfasern bei Reizung mit Inductionsschlagen. *Pflüger's Arch.*, Bd. V, pp. 31 ff.

[2] Naunyn: Ueber d. Auslösung v. Schmerzempfindung durch. Summation sech zeitlich folgender sensibles Erregungen. Arch. f. exper. Pathol. u. Pharmakol., Bd. XXV, pp. 272 ff.

[3] Gad and Goldscheider: Ueber d. Summation v. Hautreizen, Bd. I, Gesammelte Abhandlungen, pp. 397 ff. Leipzig, 1898.

sation at different times with the same number of stimuli. This variability, however, belongs only to a limited sphere. If the number of stimuli is large the variations of intervals must be slight. If the intervals are long the number of stimuli for optimum results must be almost constant. Goldscheider formulates the following law: "With increasing stimulus interval the number of single stimuli required for clearness of secondary sensation decreases: with decreasing interval the stimulus number must increase." The latency time of the after-sensation depends less upon the kind and intensity of stimuli than upon the velocity with which they succeed one another. In this respect the after-sensation is similar to the reflex movement produced by the summation of sensory stimuli.

While Goldscheider's explanation is on the ground of summation his scheme is somewhat different from that of those who attribute the phenomena of summation and inhibition to the interference of waves of excitation or of nerve force within the nerves themselves. For him the gray matter of the cord is the summation tract. When an excitation arrives from the periphery a part is transmitted directly to the centre of consciousness and a part to the cells in the gray matter described by Kölliker.[1] In these cells he supposes a change in the state of excitability. The excitation is stored up here to be discharged later and transmitted to the centre of consciousness when it is recognized either as pain or as a secondary sensation of the same quality as the primary.

IV. Vascular Tonus.

As we saw in Section I the conception of a reflex vascular tonus originated in 1852 with Claude Bernard who discovered the vaso-constrictor fibres, by the immediate influence of which the walls of the blood vessels were supposed to be kept in a tense condition. A few years later Schiff believed that he had demonstrated the presence of vaso-dilator fibres in the cervical sympathetic of a rabbit[2] and again Bernard somewhat later believed he had still more conclusively demonstrated their existence. But with this the question was not decisively settled and it has been discussed pro and con until the present time when McKendrick asserts his belief in some sort of inhibitory mechanism which accounts for the dilation of the vessels,[3] and Dastre and Morat, Howell and Porter find for interference with peripheral ganglionic mechanisms, and for antagonistic fibres

[1] Kölliker: Ueber d. feinern Bau d. Rückenmarks, Sitzungsber. d. phys. med. Gesells. z. Würzburg, 1890, p. 40.
[2] Schiff: Berner Schriften, 1856, p. 69 ff.
[3] McKendrick: Textbook of Special Physiol., 1889, p. 296.

within the nerve trunks.[1] Until, however, these fibres of opposite function can be positively demonstrated, it is simpler and more satisfactory, if we must take sides, to give preference to the theory of Dastre and Morat, and thus bring the constriction and dilation of the blood vessels into analogy with the excitation to, and inhibition of, those reflexes which are more familiar to us.

Besides this question of constrictor and dilator fibres there was another of considerably greater interest to the physiologists of the early part of our half century. Such fibres once granted, from what anatomical points in the central nervous system is the vascular system controlled?

These were sought with great zeal in the spinal cord; and Schiff first clearly stated that the vascular centres are located in that part of the nervous system. It is hardly within the scope of this paper to enter into the anatomical details. Suffice it to say that Lister, Goltz, Owsjannikow, Ludwig, Gaskell, and others of no less repute, have throughout laborious years sought for regions of predominating influence upon the circulation of the blood, and that to-day, with the addition of some details, our knowledge of this matter is practically as Lister left it in 1858: in a word that the tonus of the blood vessels depends upon the cord and local ganglia.[2]

It is of more importance for our purpose to know whether these changes in vascular tonus are reflex or automatic.

The supposition of the reflex character of the vascular phenomena has much in its favor. The experiments which were made to demonstrate the existence of vaso-constrictor and vaso-dilator fibres point in that direction; but these are not all. It had early been observed that if one hand be immersed in cold water the temperature of the other falls from one to twelve degrees C. In many such cases the temperature of the mouth and axilla rises slightly indicating that the temperature of the body as a whole is not altered. This observation was originally made by Brown-Sequard. Later he and J. S. Lombard performed confirmatory experiments. Cyon also, in 1866, declared that he had proved that the vascular centre in the *medulla oblongata* can be influenced by impressions from the periphery, *i. e.*, in a reflex manner.

The experiments by Brown-Sequard and Lombard suggested more extensive tests which were made by Putnam on frogs in 1870.[3] These experiments are of importance because they

[1] Dastre: *Arch. d. Physiol. norm. et pathol.*, 1882, p. 190.

[2] Lister: Philos. Transac's, London, 1858, pp. 607 ff.

[3] J. J. Putnam: A report of some Experiments on the Reflex Contractions of the Blood Vessels. *Boston Med. Jour.*, Vol. LXXXII, 1870, pp. 469–472.

show so clearly the reflex function of the central nervous system in relation to the blood vessels. The web of one hind foot, after the removal of the *medulla oblongata*, was watched under the microscope while the other was stimulated mechanically and chemically. Contraction of the vessels on the side opposite the stimulation is not absolutely invariable, but sufficiently so after all causes of variation have been taken into account, to make the assertion safe that the phenomena fall under the category of ordinary reflexes.

Putnam's conclusions fall in with an observation made by Callenfels and confirmed also by O. Bezold. Callenfels says:[1] "If one cuts the spinal nerve (to the ear) in a rabbit, an operation which causes a slight dilation in the blood vessels in the ear of the same side, one can afterward provoke a contraction of these same vessels not only by galvanizing the peripheral end of the cut nerve, but also by irritating, in the same manner, its trunk which still remains in connection with the medulla, and which is no more in direct communication with the part where the effect is produced. It must be then that an irritation has been transmitted to the cerebro-spinal axis and has there set up a nervous action which is directed toward the vessels of the ear." In this case the reflex arc between the stimulated point on the central stump and the vascular area where the effect is produced, is composed of the central stump of the severed nerve, the cord or spinal ganglion and the sympathetic fibres leading from this centre to the walls of the vessels. Much later than this Moritz Nussbaum expressed his belief in the possibility "that many sensory nerves are able reflexly to widen the arteries directly through the excitation of inhibitory centres or the relaxation of vaso-motor centres."[2] Further he says that from his investigation "it follows that the vaso-motor centre extends through the cord into the *medulla oblongata* and can be set into heightened reflex activity by all sensory nerves."

But the matter is by no means a simple one, for vascular variations occur also in the absence of the entire central nervous system, as was early shown by Lister[3] and others, given a crucial demonstration by Gergens and Werber[4] and reconfirmed by Gley in 1844 and by Goltz and Ewald[5] in 1896.

It may now be asked: What are the stimuli—the first term of the reflex series—which in normal life provokes vascular

[1] Callenfels: *Zeitsch. f. rat. Med.* Bd. VII, 1855, p. 157.
[2] Nussbaum: Ueber d. Lage d. Gefasscentrums. *Pflüger's Arch.* X, 1875, pp. 374 ff.
[3] Lister: *op. cit.*, pp. 617 f.
[4] Gergens and Werber: *Pflüger's Arch.*, Bd. XIII, 1876, pp. 44–61.
[5] Goltz and Ewald: Der Hund mit verkürztem Rückenmark. *Pflüger's Arch.*, Bd. LXIII, 1896, pp. 362–401.

tonus and the variations in the lumen of the vessels? It must be answered that they cannot be pointed out with certainty. Not a few of the older investigators preferred indeed to escape the need of pointing them out by calling the control automatic, though the present tendency is toward a reflex explanation, with considerable variety of opinion as to just what the stimuli are and just how they are applied.

Taking the tonus question as a whole, we see in the course of the investigations that from simple beginnings the conception of a complicated reflex mechanism has been developed. Early in the period there was a tendency to consider the vascular centres automatic, but as knowledge of the effect of both external and internal stimuli has increased the automatic theory has been given up, and co-ordinately with the growth of the reflex idea has risen also the idea of the importance of the entire organism in vascular control as opposed to that of any particular parts.

V. Muscular Tonus.

In the first section of this paper it was pointed out that the early physiologists represented by Müller and Henle had, with some exceptions, including Ed. Weber, declared their belief in a muscular tonicity dependent upon the central nervous system. Such a tonus they, as we to-day, defined as a permanently contracted state of the muscular fibres. One's characteristic physiognomy, which in popular thought is supposed to have a psychic substrate, they attributed to the state of tonus of the facial muscles. This was Henle's idea. But Wundt, much later, in a brief period of reaction from the tonus theory, declared that this can be more easily explained on the ground of temperament and the fact that the muscle substance is not completely elastic.[1]

In the later 50's the question whether there is a muscular tonus independent of mere muscular elasticity was in an unsettled state. Nor was such a term generally accepted by physiologists in the description of muscular phenomena until much later. By exact measurement Heidenhain,[2] in 1856, failed to find any lengthening of muscles after their nerves had been cut, neither did Auerbach in the same year.[3] In the face of this negative evidence, offered by men of such authority in physiology, who were two years later reinforced by Schiff,[4] the

[1] Wundt: Muskelbewegung, 1858, pp. 45 f.
[2] R. Heidenhein: Hist. u. Exp. über Muskeltonus. Physiol. Studien, Berlin, 1856, p. 19 ff.
[3] Auerbach: Ueber Muskeltonus, Jahresb. d. schles. *Ges. f. vaterl. Cultur*, 1856, pp. 127 ff.
[4] Schiff: Muskel. u. nerven Phys., 1858, pp. 30 ff.

phrase "muscular tonus" was fairly on its way to oblivion. "Certainly," says Wundt, "in the natural sciences we may explain phenomena by analogy with others better known. But when the number of natural forces is increased by way of explanation we are justified in mistrust and in criticism especially when no convincing proof is adduced for the creation. In the case of muscular tonus such proof is by no means furnished. We can explain the tense state of muscles without going beyond what we know to exist."[1] The constant tension of muscles, he is convinced, can be explained by elasticity.

But the fundamental experiment was yet to be made. This was performed by Brondgeest in 1860.[2] Though his earlier experiments had led him to negative results, the one in question he thought required the assumption of a muscular tonus. The sciatic nerve on one side of a spinal frog was severed and the animal was suspended by the jaw. At once the foot on the opposite side was found to form the greater angle with the median line of the body. Furthermore he concluded that this tonus is a reflex since section of the posterior roots produces precisely the same phenomena as severing the sciatic.

This work of Brondgeest's called forth many repetitions of the experiment in different forms. Hermann assumes a spinal sensorium.[3] The frog is trying to hold his foot up in its normal position when at rest and he "holds it at such a height as he can endure without exhaustion."

Cohnstein observed that a frog prepared according to Brondgeest's method, and placed in a horizontal position upon a surface of quicksilver shows no difference whatever in the position of its limbs.[4] Brondgeest's tonus is therefore due, he thinks, to the stimulus afforded by the traction of the muscles upon the skin. It is a reflex: not necessarily a constant state of the muscles in the sense of Müller and Brondgeest. Neither is it necessarily uniform all over the body. Here a group of muscles may be tense, there another flaccid. The difference between this explanation and that offered by Lombard in 1896 is only a matter of words.[5] Lombard says: "If, when one is quietly sitting, one turns one's attention to the sense impressions coming from clothes, warmth, sight, hearing, one is convinced of the multitude of impressions exciting the nervous

[1] Wundt: Muskelbewegung, 1858, p. 45.
[2] Brondgeest: *Arch. f. Anat. u. Physiol.*, 1860, pp. 703 f.
[3] Hermann: Beiträge zur Erledigung d. Tonusfrage. *Arch. v. DuBois u. Reichert*, 1861, pp. 350 ff.
[4] J. Cohnstein: Kurze Uebersicht d. Lehre vom Muskeltonus. *DuBois u. Reichert's Arch.*, 1863, pp. 165 ff.
[5] Lombard: Art. Am. Text B. of Phys., 1896, p. 134 ff.

system. The effect of all this is to cause motor cells to send delicate excitations to the muscles. Hence muscular tonus." Yet more convincing than any other argument for the negative was that which Schwalbe proposed after a long series of experiments.[1] Brondgeest's phenomena, he says, are due to an increased elasticity of muscles which remains behind after vigorous action and only gradually passes away. These experiments led G. Heidenhain, in 1871, to think the whole question of muscular tonus ended. And Eckhard, too, in 1879, doubted its existence. This, however, was a mistaken position.

The evidence on the positive side of the question is really preponderant and, Ferrier says,[2] in speaking of the work of Cyon and Tschirjew: "The negative results of previous experiments must, therefore, be attributed to faulty methods, and the existence of a muscular tonus, of a reflex character at least, must be considered as proven." Cyon, in 1865, showed that the excitability of the motor roots is heightened by stimulation of the sensory,[3] and several years later confirmed by exact measurement[4] the results of Steinmann who found that section of the posterior roots is followed by lengthening of the muscles. A little later Tschirjew demonstrated the same phenomena with equal exactness.[5] Thereafter there were a few isolated attempts to disprove the theory, and some investigators even in the face of these accurate scientific measurements continued to doubt; but it was not quite in accord with the facts when, in 1887, Westphal said that at that time physiologists were not inclined to admit the existence of a muscular tonus. At the present time the consensus is general in favor of the tonus theory.

Let us now turn to the question of how the tonic state is maintained. The arguments for an automatic tonus have not received very extended consideration though Foster says[6] that "it seems a matter of words whether we speak of it as an automatic or a reflex action of the cord. We may distinguish the part played by an afferent impulse in maintaining tonus from the part played by it in causing reflexes. In the former it is analogous to a supply of arterial blood in maintaining an adequate irritability of nervous substance. In the latter the impulses lead directly to a discharge of energy. And it is con-

[1] Schwalbe: Pflüger's Untersuch. aus d. Bonner phys. Lab., 1865, pp. 64 ff.
[2] Ferrier: Functions of the Brain, 1886, p. 82.
[3] Cyon: Ber. d. sächs. Ges. d. Wissensch. Math. Phys. Cl., 1865, Bd. XVII, pp. 85 ff.
[4] Cyon: *Pflüger's Arch.*, Bd. VIII, 1874, pp. 347 ff.
[5] Tschirjew: *Arch. f. Anat. u. Phys.*, 1879, pp. 78 ff.
[6] Foster: Textbook of Phys., Vol. III, p. 925, 1892.

venient to call the two by different names." It is, however, conceded that the tonus of the muscles of the blood vessels and the sphincters is automatic.

That muscular tonus, especially of the skeletal muscles, can be affected reflexly does not admit of a doubt. Sherrington cut the nerve leading to the hind leg of a cat.[1] When he stimulated the central stump the tonus of the extensor was lowered and the lower shank hung down straight. This phenomenon comes under the formula for reflex actions involving the spinal axis. Wundt says:[2] "The tonic excitations of the skeletal muscles appear to be of an exclusively reflex nature, since section of the muscle nerves aside from the accompanying quivering and elastic after-effects produces no change of muscular tension." But the arguments for automatism aside, and the reflex nature of the phenomena once granted, the question remains as to the organs mediating the stimuli for arousing the reflex.

As stated above Cohnstein in 1865 believed that the stimulus is afforded by the traction of the skin by the weight of the muscles. This theory for a long time received the most complete acceptance. But even where the skin lies in loose folds, without any tension whatever under the weight of the muscles, the latter are in a state of tonus. Experiments on frogs have proved that large portions of skin may be removed from the body without prejudicing muscular tonicity, or even interfering with co-ordinated movement which is so much under the influence of the tonicity of the muscles.

Mommsen, in 1885, reviewed arguments and experiments to show that the constant tension of the muscles is not, as Eckhard says,[3] entirely dependent upon afferent cutaneous impulses.[4] That afferent roots, however, have a function in connection with the phenomena in question, is beyond a doubt. The blocking of all afferent channels in a limb lowers the tonus of its muscles. Such a limb becomes excessively mobile and *rigor mortis* is delayed in its muscles.[5]

But how much of this result is due to the section of cutaneous afferent nerves cannot be determined. Since, as already stated, removal of an animal's skin does not cause tonus to cease entirely this state must be due in part to influence from the mus-

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[1] Sherrington: On Inhibition of the Tonus of a Voluntary Muscle by Stimulation of its Antagonist. *Jour. of Physiol.*, Vol. XXIII, Suppl. 1898, p. 26.

[2] Wundt: Physiol. Psych., Bd. II, 1902, p. 254.

[3] Eckhard: Hermann's Handb. d. Phys., Bd. II, pp. 269 f.

[4] Mommsen: Beitrag z. Kenntniss d. Muskeltonus. *Arch. f. Path. Anat. u. Phys.*, Bd. 101, pp. 22 ff.

[5] Sherrington: Schäfer's Phys., Vol. II, p. 799.

cles. Tschirjew, whose work in connection with Cyon's is so important for this subject,[1] called attention to the fine network of intrafibrillar nerves within the muscles, and believes that these are the end organs for muscular tonus, and the centripetal nerve fibres of the muscles. The stretching of these fibres by the weight of a dependent limb, their compression by the muscles, or their disturbance by the activity of the muscles, he thinks, furnishes the stimulus for reflex tonus. Tschirjew relates some experiments with dogs in support of his theory. Such an animal is put upon his back so that his legs of their own weight are bent upon his body. The limbs are soon found to be asleep: *i. e.*, the muscles are toneless. Like phenomena under similar circumstances can be observed in men. This, Tschirjew attributes to the quiet unstretched condition of the muscles so that the nerve fibres in the aponeuroses are undisturbed.

It is impossible to demonstrate that either muscle or skin has a controlling interest in the production of tonus. By cocainization or flaying the effect of cutaneous impressions can be ruled out, but it is not possible to dispose of the muscular nerves while leaving the muscles intact.

VI. Tendon Reflexes.[2]

Closely related to muscular tonus are the tendon reflexes. In its absence they do not appear. Hence the production of knee jerk is a practical means for demonstrating the tension of muscles.

Scarcely three decades have elapsed since these phenomena were first described, but their great practical importance in the diagnosis of certain spinal diseases justifies the voluminous literature of a descriptive and experimental character which has appeared in this field within so brief a period. The tendon reflex is rarely absent in health but it is entirely abolished in tabes dorsalis, locomotor ataxy, and atrophic paralysis, and increased in diseases of the lateral columns. Further it has been supposed[3] that these phenomena have much influence in the purposeful management of movements, and that in men they are of importance in the maintenance of equilibrium.

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[1] Tschirjew: *op. cit.*

[2] For the material of this section I am largely indebted to the exhaustive account of the subject found in (Hermann Netter's Inaug. Dissert.: ("Geschichte d. Lehre vom Knie-phänomen." Freiburg, 1897.) The citations herein, excepting where exact page references are given, are *copied directly from Netter's work.*

[3] Saureys: Quelques réflexions sur le raison physiologique et la localisation probable du réflexe patellaire. Ref. Annales de la Soc. Belge de Neurol. Ve année.

The knee jerk, produced by smartly striking the patellar ligament of a dependent limb, is a typical instance of the phenomena. On such a stimulus the lower leg is involuntarily thrown forward. The jerk is by no means confined, however, to the knee. It may be produced by a similar stimulation of any ligament in the body.

The phenomenon was first described by Erb[1] who believed that it was a true reflex, analogous to those excited by cutaneous stimulation, and dependent upon an entire reflex arc. Tschirjew was the first to investigate it experimentally, and he, too, decided in favor of the reflex theory.[2] Westphal, however, looked upon it as a direct muscular contraction induced by the sudden tension occasioned by the blow upon the tendon.[3] Erb and Westphal, therefore, represent opposite poles of thought on this question and about them many supporters have rallied. The disciples of Westphal are found chiefly among English and American scientists, those of Erb among the Germans. Erb and Westphal were of the same opinion only in this,[4] that the tendon phenomena do not depend upon cutaneous impressions.

We shall first speak of the supporters of Erb and the reflex theory. Tschirjew has already been referred to as a member of this school, and the first experimental investigator of the tendon phenomena.[5] He found that the knee jerk disappears when the cord is cut transversely between the fifth and sixth lumbar vertebræ in puppies. This observation was later confirmed by Senator who found that hemisection of the cord displaces the phenomena only on the side of the cut.[6] Another indication, from the standpoint of the anatomist, of the reflex nature of the knee jerk is found in the fact, to which Sachs called attention, that the tendons themselves contain nerves.

But more convincing than any anatomical evidence are the experiments by Schultze and Fürbringer, who published their work in 1875. In one experiment on puppies the cord was cut in the upper dorsal region and the quadriceps muscle was severed from the patella. The disconnected muscle was then held above the bone, so as not to be affected by its jarring, and slightly stretched. It responded, by slightly twitching, to every stroke upon the opposite patellar ligament, but after either left or right crural nerve was severed the phenomenon

[1] Erb: *Arch. f. Psychiatrie*, Bd. II, 1875.
[2] Tschirjew: *Arch. Psychiatrie*, Bd. VIII.
[3] Westphal: *ibid.*, Bd. V.
[4] Tschirjew: *op. cit.*
[5] Tschirjew: *op. cit.*
[6] H. Senator: Ueber Schnenreflexe u. ihre Beziehungen z. Muskeltonus. *Arch. f. Anat. u. Physiol.*, 1880, pp. 197 ff.

could no more be produced in any way. Their conclusion is that in the tendon phenomenon we have not to do with a mechanical muscular contraction mediated directly through the tendons, but with a reflex mechanism whose arcs for the lower extremities pass through the lower parts of the cord. Gowers, Buzzard, Prevost and others also have decided in favor of the reflex theory, and Sternberg, in a series of articles,[1] makes the evidence practically decisive. He shows that a muscle may be severed from its ligament, lifted from the bone and turned back into a reversed position without influencing the tendon phenomenon.

Of another nature are the experiments by Tschirjew, Rosenheim and others. They measured the time of latency between stimulus and knee jerk and compared it with the latency period in recognized reflexes and in direct muscle stimulation. The time measured by Tschirjew is too long to lend color to the theory of direct muscle stimulation, and Rosenheim[2] never found a shorter period than .025 second: this and even greater periods corresponds to the latency times of undoubted reflexes. Rosenheim further computed the distance from the knee to the spinal cord and concluded that the latency time found by him is about such as should be expected under the conditions of an ordinary reflex.

Of the same character as these are a number of experiments performed by A. D. Waller which are construed as support for Westphal's theory of direct muscle stimulation.[3] He found that the latency time of the knee jerk in men is about .03 seconds, which, he says, is so much shorter than the latency of a true reflex that this phenomenon must be considered a result of direct muscle stimulation. But since the latency of the lid reflex is only .05 seconds (Exner) the difference is hardly sufficient to be convincing. Of more significance for this theory are his experiments on puppies. With direct electrical stimulation of the *rectus femoris* the latency time is .0076 second. The tendon phenomenon of the same muscle is .008 second, while the time between a cutaneous stimulus and a true reflex he gives as .033 second. Obviously the meaning of this comparison depends upon the place and conditions of cutaneous

[1] Sternberg: Sehnenreflexe bei Ermüdung, *Centbl. f. Phys.*, 1887; ditto: Ueber Sehnenreflexe, Verhandl. d. Cong. f. innere, Med., 1890; ditto: Monogr. Die Sehnenreflexe, etc., Leipzig, u. Wien, 1893.

[2] Rosenheim: Exper. Unters. d. unter den Namen "Sehnenphänomene" bekannten Erscheinungen unter möglichsten Berücksichtigung vom Versuchen am Menschen u. patholog. Unters., *Arch. f. Psychiatrie u. Nervenheilk.*, XV.

[3] Waller: On the physiol. Mechanism of the phenomena termed "Tendon Reflexes." *Jour. of Phys.*, XI, pp. 384 ff.

stimulation. In another communication[1] he compares the distance of the gastrocnemeus and rectus from their spinal centres as ½m— 1m. The delay in the peripheral nerve, therefore, according to his computation, if the phenomenon is a reflex, should be .02-.03 second and .04-.06 second, plus the central and muscular latencies. As a matter of fact, however, they are about the same: .03-.04 second. De Watteville also argues for the direct stimulation theory on the ground of difference in latency times.[2]

It is unnecessary to go into the details of other analogies between the tendon phenomena and recognized reflexes. Suffice it to say that Sterling and Kronecker, Rosenheim, Jarisch and Schiff[3] all find that the knee jerk is subject to the law of summation of minimal stimuli and that Prevost and others have produced the crossed phenomenon.

The reflex theory, indeed, is in so general favor that we may fairly cease speaking of "tendon phenomena" and substitute therefor "tendon reflexes."

VII. Direction of Transmission and Co-ordination of Reflexes.

It was pointed out in the first section of this study that Pflüger's Laws represent the last important stage in the development of our knowledge of reflexes in the period previous to that which we are reviewing.[4] It is now our task to see how these laws have stood the test of fifty years and in connection with them to introduce some other matter of wider general interest.

The laws were for a long time generally accepted. But there are exceptions to each of them and in the light of present knowledge they hardly merit the importance that has been given to them. The contraction of muscles does not always occur on the side of the stimulus. It often occurs on the opposite side, depending upon the intensity of the stimulus—a contradiction of the first law. It is not always true that a unilateral reflex, if it spreads, involves the symmetrical opposite muscles. In the course of numerous experiments on dogs, Goltz was impressed with the vigor with which diagonally crossed reflexes occur after the cord has been separated from the inhibitory influence

[1] Waller: Muscular Symptoms known as Tendon Reflex. *Brain*, Vol. III, pp. 179 ff.

[2] de Watteville: On Reflexes and Pseudo-Reflexes., *Brit. Med. Jour.*, May 20, 1882.

[3] Jarisch u. Schiff: Unters. über d. Kniephänomen. *Weiner med. Jahrb.*, 1882.

[4] A statement of these laws is given by Dr. Hodge in his sketch of the earlier history of reflexes. *Am. Jour. of Psy.*, Vol. III, pp. 359 f.

of the brain.[1] Gergens also, working with dogs, found the same crossed phenomena.[2] If the left side of a spinal dog's neck be lightly tickled the right hind leg performs scratching movements. This phenomenon is invariable under these conditions with dogs. That it is independent of any volitional activity on the animal's part is indicated by the fact that however vigorous are the scratching movements the animal appears totally indifferent to them. Luchsinger added to these tests a number of experiments on tritons and lizards and substantiated the earlier observations.[3] With frogs, however, he was unable to induce a crossed reflex. Again the same author performed analogous experiments on the cricket and carabus, creatures with six legs.[4] These were painted with ether and thus made reflex. By the aid of a pair of tweezers he made walking movements with the middle leg on one side of the cricket and the diagonally opposite hind limb moved likewise. The carabus, which, unlike the cricket, uses all three pairs of legs for locomotion, under a like test on a front leg reflexly moves the diagonally opposite middle leg and the hind leg diagonally opposite the latter. On the other hand, says Sherrington, "in the spinal rabbit the crossed reflex from one hind limb to the other is not an asymmetrical movement but a symmetrical flexion."[5] This, as Luchsinger saw,[6] has its explanation in the normal mode of locomotion of the animal in question. Crossed reflexes in the locomotor organs occur in animals which propel themselves as the dog and cat; symmetrical reflexes in hopping animals such as the frog and the rabbit. There are thus exceptions to the second law.

The abduction of the tail from the point attacked, when it is intensely stimulated, is a well-known exception to the third law.

And as to the fourth law, viz.: that the spread of a reflex excitation is always toward the head if a spinal nerve is originally stimulated, there are numerous contradictions. The experiments, described below, made by Mendelsohn, show that irradiation may occur from the head tailward. On this point Sherrington says:[7] "From the pinna may be excited move-

[1] Goltz: Ueber d. Verrichtungen d. Grosshirns. *Pflüger's Arch.*, Bd. XIII, p. 1 ff.
[2] Gergens: Ueber gekreuzte Reflexe. *Pflüger's Arch.*, Bd. XIV, pp. 340–341.
[3] Luchsinger: Ueber gekreuzte Reflexe. *Pflüger's Arch.*, Bd. XXII, pp. 179–180.
[4] Luchsinger: Zur Theorie d. Reflexe. *Pflüger's Arch.*, XXIII, pp. 308 ff.
[5] Sherrington: Schäfer's Textbook of Phys., Vol. II, p. 822.
[6] *Op. cit.*
[7] Sherrington: Schäfer's Physiol., Vol. II, p. 823.

ments of all the limbs, the neck, the tail, and trunk. The irradiation from this reflexigenous area usually presents the following order: 1, Neck and homonymous fore limb; 2, homonymous hind limb; 3, tail and trunk on both sides; 4, contralateral hind limb; 5, contralateral fore limb. From the fore foot can be excited, besides movements of the fore limb itself, movements in the other limbs and tail." Recently there appeared a paper by the same author which grew out of a successful attempt to find detailed evidence of aborally running reflex spinal paths.[1] He found that "Each spinal segment (in the dog) possesses a wealth of neurones with backward running axons connecting it with practically all the spinal segments behind itself,"[2]—one more piece of evidence that the fourth law does not hold good in the mammalian spinal cord. With all this, however, especially in the case of the "long reflexes," it is difficult to predict the course of an excitation. Very often it happens that, for no reason that can be discerned, the usual course of dispersion of excitation is unused and an unusual one chosen.

It appears, therefore, that different portions of the cord are, as Rosenthal thought, differently suited to the transference of reflex excitations. But this investigator believed that normal reflexes are made possible chiefly through the mediation of the *medulla oblongata* and also through the uppermost parts of the cord. The exhaustive experiments by Mendelsohn sufficiently test the latter conclusion and furnish additional contradictory evidence regarding the validity of Pflüger's fourth law. In his first communication[3] he confirms Rosenthal so far as reflexes occasioned by weak stimuli are concerned. For the effectiveness of such stimuli the integrity of the upper part of the cord is necessary. Their effect is prejudiced if a cross section of the cord be made a few millimeters above the brachial plexus, or if a longitudinal median section is made from above to the same point. These statements apply only to the occurrence of reflexes on the side opposite the stimulus which is applied to a hind extremity. The upper part of the cord is therefore more open than the lower to the transmission of excitations. In the second communication[4] the author discusses the effects of hemi and total cross sections of the cord just below the brachial plexus in the mid-dorsal region, and just above the exit of the nerves for the lower extremities.

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[1] Sherrington: Observations on some Spinal Reflexes and the Interconnection of Spinal Segments. *Jour. of Phys.*, Vol. XXIX, p. 58.

[2] *Op. cit.*, p. 96.

[3] Mendelsohn: Berlin Akad. d. Wissensch., 1882, pp. 897 ff. See also *Arch. f. Anat. u. Physiol.*, 1883, *Phys. Abth.*, pp. 282 ff.

[4] Mendelsohn: Berlin Akad. d. Wissensch., 1883, pp. 123 ff.

In the case of hemisection and stimulation on the injured side the excitation may cross from the stimulated hind leg to its opposite and from thence to the fore extremities in weakened degree. Longitudinal median section between the brachial and lumbar plexuses only weakens the reflexes, but total transverse section in the dorsal part causes reflexes in the anterior limbs wholly to disappear. This series furnishes sufficient proof that reflex excitation is not fatally confined to any definite tract in its headward progression.

In Mendelsohn's third article[1] we find again, beside a confirmation of the preceding conclusions, another contradiction of Pflüger's fourth law. The excitation following a stimulus of the right fore foot spreads toward the hind limbs although with more difficulty than in the opposite direction. Sherrington, too, has shown that in a dog with hemisected cervical cord the scratch reflex can be readily produced on the side of the lesion by tickling the shoulder or neck.[2]

Thus at different parts of the cord the ease with which the transmission of excitation occurs, is what is to be expected from the segmental character of the spinal axis in so far as it is segmental in a functional sense. Instead of a supreme centre for the co-ordination of reflexes which was once supposed to exist in the upper parts of the spinal axis, it is now held that the cord is composed of a great number of parts more or less independent and capable of caring for a definite group of reflexes. Physiologists have been gradually led to this conception of the reflex functions of the spinal cord through the long line of researches extending from those of Lister, described in the section on Vascular Reflexes, to the work of Goltz, who tells us that the clasping reflex of the male frog is possible though the whole cord save a small portion in the brachial region be removed. Later experiments on the dog by the same experimenter[3] are suggestive of a similar division of function. But to Schrader, a little earlier than this, we are indebted for far-reaching experiments and for a clear-cut statement regarding the segmental character of the cord. He says:[4] "The series of experiments we have given teaches us that the central nervous system of the frog can be divided into a series of sections, each of which is capable of performing an independent function. It brings the central nervous system of the frog into closer relation with the central nervous system of the lower

[1] Mendelsohn: Berlin Akad. d. Wissensch., 1885, pp. 107 ff.
[2] Sherrington: *op. cit.*, p. 90.
[3] Goltz: Der Hund mit Verkürztem Rückenmark. *Pflüger's Arch.*, Bd. LXIII, 1896, pp. 362 ff.
[4] Schrader: Zur Physiol. d. Froschgehirns. *Pflüger's Arch.*, XLI, 1887, pp. 75 ff.

forms, which consists of a series of distinct ganglia that are connected by commissures. It speaks against the absolute monarchy of a single apparatus, and against the existence of different kinds of centres, and invites us to seek for the centralization in a many-sided coupling of relatively independent stations."

More striking than all other reflexes, because more accessible to the unaided eye, are the co-ordinated purposive movements of spinal animals, and their so-called spontaneous movements. The beheaded eel, *e. g.*, leaps from the heated water into which it is thrown.

Long before our period it was known that the spinal frog will wipe an acidized spot on its abdomen as if to remove the offending substance. On the other hand if the same spot be pinched by a pair of tweezers, one or both hind feet are used to push away the instrument: no attempt is made to stroke the injured surface.[1] This is a typical case of co-ordinated reflexes. In so far as the movement is adapted to reach an end it is purposive. The same sort of purposive co-ordination Gergens describes in the spinal dog.[2] When the breast or neck of such an animal is tickled the opposite hind limb is brought up to scratch the spot. Apparently, however, these movements are not always purposive. For instance a spinal dog resting upon his fore legs extended, and lying upon one hind hip—the familiar position of a resting dog—when stimulated in the way just described, often moves his hind leg in such a direction that it cannot reach the stimulated spot. Yet Gergens in an attempt to defend the purposive character of even such movements, says:[3] "In view of these facts I may assume that the new excitation centripetally conducted during the performance of the reflex act of movement, even without the end aimed at, to wit: the removal of the stimulus, is sufficient to weaken in a certain measure the state set up by the stimulus in the portion of the central organ in question." The same author made other experiments on the spinal frog which illustrated another case of co-ordinated purposive action. The spinal frog whose toes of one hind foot are stimulated, after a time thrusts the foot under its body as if for protection.[4]

Thus far we have spoken only of the co-ordinated movements of single members. The movements of locomotion even,

[1] Pflüger: Der sensor. Functionen d. Rückenmarks, Berlin, 1853, p. 18.

[2] Gergens: Ueber gekreuzte Reflexe. *Pflüger's Arch.*, Bd. XIV, pp. 340 f.

[3] *Op. cit.*, p. 343.

[4] Gergens: Einige Versuche über Reflexbewegung mit dem Influenz-Apparat. *Pflüger's Arch.*, XIII, 1876, pp. 61 ff.

in simple spinal animals, though imperfect are yet decidedly impressive. They are of different degrees of perfection in animals of different grades. To refer to the beheaded eel again: it is evidently able to perform co-ordinated locomotor movements.[1] Tarchanoff found that beheaded ducks for a few hours after spinal transection perform quite elaborate movements,[2] They swim, dive, steer with their tails, and attempt to fly from the water. Even after they are totally beheaded their movements are no less complex and orderly. By section at different parts of the cord the author is led to decide that the co-ordinating centre for these movements is in the lumbar portion. Whether they are purely automatic or reflex is a matter he cannot decide. The spinal frog, too, Sherrington says[3] is capable of co-ordinated locomotion. "In the spinal frog placed in water warm enough to form a skin stimulus, *e. g.*, 36° C, I have seen co-ordinate swimming, for a short time vigorous, and executed with the normal bilateral stroke of the hind limbs. But the total co-ordination is distinctly less good than when metencephalon and myelencephalon remain. There is a tendency for the animal to dive deeper and deeper in the water: this seems due to the sunken position of the head: the loss of the semicircular canals cannot be without importance for this condition." The spinal turtle and the snake by proper stimulation move forward co-ordinately,[4] and Bickel confirms Sherrington and others in the declaration that the spinal frog can either leap or swim.[5] These phenomena occur, however, only in cases extremely successful in an operative sense. Such examples point to a vast functional complexity in the spinal cord.

Considerable attention has been given to attempts at a physiological and anatomical explanation of spinal co-ordination. Sanders-Ezn in 1867 concluded, as a result of acid stimulation of different parts of the bodily surface, that a definite movement is produced by the stimulation of every distinct point of the skin.[6] With equally intense stimulus an almost constantly typical form of movement is produced. Sherrington says that he has seen the point of stimulation profoundly influence the reflex reaction. "Of the four senses which pre-eminently furnish space perception, one has its end organs in the skin, an-

[1] See Bickel: Ueber einige Erfahrungen aus d. vergleichenden Physiol., etc., *Pflüger's Arch.*, Bd. 83, pp. 155-159.

[2] Tarchanoff: Ueber automatische Bewegungen bei enthaupteten Enten, *Pflüger's Arch.*, Bd. XXXIII, pp. 619-622.

[3] Sherrington: Schäfer's Phys., Vol. II, p. 818. See also: The spinal Animal, *Med. Chir. Trans.*, Vol. LXXXII, p. 468.

[4] Bickel: *op. cit.*, p. 160.

[5] *Op. cit.*

[6] See Gergens: *Pflüger's Arch.*, Bd. XIII, p. 61.

other in the musculo-articular structures, and these are therefore in great measure spinal senses. It is not surprising, then, that *per se* the locus of the stimulus is for a spinal reflex an important determinant of the resulting movement."[1] But Gergens earlier[2] was led to believe that the locus of the stimulus is of less moment. When the toe of a spinal frog was lightly stimulated by electricity the foot was thrown out and drawn back to a different position. After stimulation at the same point a second and third time, etc., it was drawn back always to a new position and finally thrust under the body as described above.

The internal mechanism for co-ordination is yet a matter for speculation. It is more fruitful for us to inquire regarding the co-ordinating mechanisms in the large. When the bulbo-spinal frog is turned upon his back he rights himself. In explanation of such phenomena as this Steiner supposed a "righting centre."[3] But as Loeb justly says:[4] "He does not consider the possibility that contact stimuli and the irritable structures at the periphery may be sufficient for this reaction." Further he suggests that if we grant a "righting centre" in this case we shall be tempted to posit a "flying into the flame centre" for the moth. Thus the positing of centres can go on to infinity.

The theory proposed by Longet in 1842 in the main is still current.[5] "The primary condition of harmony in movements is found in the very sensation of their accomplishment. In effect how should one suppose that a man or an animal who has lost the sensation of movement executed by its members, and is not able to judge of their position, of their connection with external objects, and does not even know, so to speak, that they exist, and finally does not sense with its members the ground on which it rests, how should one suppose that such an animal is able to walk regularly, to preserve its equilibrium and to do this with energy, promptitude, and primary harmony? In this case the will has only a very incomplete effect upon the muscles: for one must not be surprised at the considerable disturbance which a profound lesion of the posterior medullary fasciæ occasions in the locomotor functions, since these fasciæ preside exclusively over sensibility." Claude Bernard in 1858 furnished strong proof for the correctness of Louget's theory.[6] Want of purposiveness in movements follows section of the sensory nerve.

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[1] Sherrington: Schafer's Phys., Vol. II, p. 832.
[2] *Op. cit.*
[3] Steiner: Central Nervous Systems, I, 1890, p. 39.
[4] Loeb: Phys. of the Brain, 1900, p. 182 (note).
[5] See *Pflüger's Arch.*, Bd. XXXVII, p. 618.
[6] Claude Bernard: Leçons sur la phys. et la pathologie du système nerveux, I, 1858, pp. 246 ff.

While this view is in the main yet held as correct, it has been somewhat modified. Talma, for example, has conclusively shown that of the sensory nerves those of the muscle sense are vastly more important for co-ordination than the cutaneous nerves.[1] If the posterior spinal roots of a frog be severed it is able to move clumsily if at all. If only the skin, on the other hand, be removed, co-ordinate locomotion still occurs. When the posterior roots from one limb are cut and the opposite normal limb is stimulated the injured one moves, if at all, very slightly and without the character of purposiveness. Such an operation has the same effect before as after decapitation. But after section of the cord movements occur in less degree. Hence he concludes that reflex movements depend not only upon the intensity of the stimulus and not alone upon the integrity of the motor tracts. In his experiments these were left intact, and yet section of the sensory nerves effected a considerable modification of these muscular effects. The law of Longet cited above he modifies as follows:[2] "Because the interruption of the function of the sensory nerves very profoundly influences the movements arising in the cord, a chief condition for the harmony of movements, even for the cord, must be laid in the perception of the movement occurring. Because a cord which has lost the sensation of movements does not permit the muscles to move regularly and with their former energy, precision, and harmony, it must normally judge the position of the peripheral parts and their relation to the external world in about the same manner as the brain."

"The fact that the reflex movements, under the dominion of the cord, arising in the limbs without feeling, are much less purposive than in the normal limb, proves that the idea of the presence of a part is an important condition for the delivery through the cord of motor impulses for this part."

In Talma's work we have evidently a recurrence to the notion of a spinal cord soul which co-ordinates the movements of spinal animals. This doctrine, as we have said, was stoutly defended by Pflüger. It has occasionally arisen again since the beginning of our half century, notably in the minds of Gergens, Luchsinger, Lewes and Lange.

VIII. Theoretical Considerations.

Pflüger ascribed the movements of spinal animals to the control of a part of a divisible soul. The purposive co-ordinated movements of reflex preparations were adduced as support for this

[1] Talma: Eine psy. Function d. Rüchenmarks, *Pflüger's Arch.*, Bd. XXXVII, pp. 617-623.

[2] *Op. cit.*, p. 621 f.

view.[1] Lotze opposed this doctrine. The spinal animal for him was a machine, but not of the first construction. Those of a scientific turn of mind eagerly grappled with this idea because it promised to make it possible for physiologists to approximate toward exact scientific research. If Pflüger's theory were correct physiology would be difficult as a science. The influence of an independent soul changing of itself and moving things, could hardly be calculated.

Goltz demonstrated to his own satisfaction that the brainless frog does not possess a soul in the sense in which Pflüger uses the term: namely, a deliberative consciousness.[2] He supports the Lotzeian theory.[3] His experiment with the normal and the beheaded frogs in gradually heated water,[4] attracted wide attention because by many it was regarded as proof positive of an unconscious spinal cord. The beheaded frog rests quietly and allows itself to be cooked whereas the normal animal leaps from its danger.

Wundt, however, while admitting the value of this test, says that it does not finally prove the lack of consciousness.[5] No objective test can inform us of the presence or absence of consciousness in another individual. The strongest proof for the cord's unconsciousness lies in the fact that in our hours of sleeping we move our bodies without awareness at the time and the act is not subject to recall. To this the objection is raised that a dull consciousness such as the cord possesses must have a short memory. Further, says Wundt:[6] "If, therefore, after the loss of the brain, movements remain which have the completest similarity with voluntary movements, it does not follow that in fact these are voluntary movements. If they are instinctive activities, however, they rest just as little and as much upon pure mechanism as the voluntary movements themselves. This distinction of mechanism and pure psychic activity is, so soon as one grants that consciousness is a product of the development of the unconscious soul, no more valid." These movements are instinctive. In essentials Wundt agrees with Lotze, but takes exception to the Lotzeian declaration that an animal body whose soul has had no experiences could not perform movements after decapitation. Says Wundt:[7] "We may, resting upon the law of heredity,

[1] See also Hermann: Archiv v. DuBois u. Reichert, 1861, pp. 350 ff.
[2] Goltz: Nervencentren d. Frosches, 1869, p. 100. See also Königsberger med. Jahrb., Bd. II, p. 189.
[3] Goltz: Nervencentren, etc., p. 82.
[4] Goltz: Nervencentren, etc., pp. 127 ff; see also Königsberger med. Jahrb., Bd. II, p. 218.
[5] Wundt: Menschen- u. Thierseele, Bd. II, 1863, pp. 427 ff.
[6] *Loc. cit.*, p. 433.
[7] *Loc. cit.*, p. 434.

express the supposition that what enters ready made into the life of the single cell is a product of the development of preceding generations, so that the arousal of those purposive and unconscious movements can be derived from a practice which is not limited to the individual life." Wundt believes there are grades of consciousness, and even in acephalous animals, consciousness "has developed co-ordinately with the whole body. He says:[1] "The soul is separable and must be in so far as it consists in a series of separated functions."

Other support for the Lotzeian theory is furnished in the experiments by Steiner on the beheaded shark. The fish is first operated upon so that in swimming it performs circular movements. Ten hours afterward when it is beheaded it continues to swim in a circle. This, he thinks, is the after effect of the former experience.

But before going on to the more purely mechanical views we must attend to a strong reversion toward Pflüger's theory.

G. H. Lewes proceeds from the assumption that identity of tissue carries with it identity of physiological property, and that similarity in the structure and connections of organs involves corresponding similarity in function.[2] The histological identity of the brain and cord is more and more confirmed by microscopic investigations. In the concluding paragraph of the article cited he distinguishes between spinal soul and sensibility: "In conclusion," he says, "let it be observed that unnecessary obstacles are thrown in the way of rational interpretation when connotative terms such as spinal soul are adopted. It is one thing to assign a general physiological property, such as sensibility, to the nervous centres; another thing to assign a term which is the abstract expression of the connexus of sensibilities to any one centre. In saying that the spinal cord is *a* seat of sensation, it is not meant that it is *the* seat, nor that the sensations are specifically like the sensations of color, of sound, of taste, of smell; but they are as like these as each of these is like the other." Nowhere does Lewes define himself more clearly.

His views are more fully elaborated in the Physical Basis of Mind.[3] Objectively a sensation is a phenomenon of movement but it is separated from other phenomena by the specialty of its conditions. It is not purely mechanical. It has the character of selective adaptation which separates it from the movement of machines: that is, it combines motor impulses to suit the varying requirements of the effect to be produced. Sensibility,

[1] *Loc. cit.*, p. 436.
[2] Lewes: Sensation in the Spinal Cord, *Nature*, Vol. IX, pp. 83 f.
[3] Lewes: Physical Basis of Mind, 1877.

on the other hand, represents the property of grouping and combining stimulations.[1] The only ground for denying sensation to the cord rests in the assumption that the brain is the sole seat of sensation.[2] The lack of uniformity in reflex actions he argues is evidence of sensation. It belongs to every segment.[3]

But sensation has two opposite faces: the objective and the subjective. On the subjective side it is consciousness. Since we know that certain actions are consequent on certain perceptions we are justified in inferring that whenever the actions are performed, the perceptions preceded them. That the perception may have stimulated the action and yet been unaccompanied by consciousness is not evidence to the contrary. We read without consciousness of the separate letters and yet are not reading automata. So far as these actions are dependent upon vital processes they are not expressible in mechanical terms. Vital facts, especially facts of sensibility, have factors neither discernible in machines nor expressible in mechanical terms.[4] According to Lewes' theory, so long as neural processes continue in the separated parts of the body we may speak of a soul separable with the body.

Talma sees the reflex phenomena in the same light as Lewes. After reviewing his experiments on the co-ordination of reflexes he says:[5] "These experiments appear to me to furnish indisputable proof for the existence of a spinal cord soul in so far as one understands thereby a complicated action in the nerve cells, as it is generally assumed in the cells of the brain,—one which determines the kind and the mode of movements following upon sensory stimulation." Luchsinger before him had called attention to his own observation that the spinal frog will turn toward a gentle stimulus but away from a severe one and concludes:[6] "The desouled animal knows how to adjust itself with the greatest nicety to external circumstances." Consciousness, says Maudesley,[7] attends the formation of neural plexuses. It is our business to investigate the conditions not of a general consciousness but of a number of particular ones. And Marshall utters a similar view when he says:[8] "Only the

[1] *Loc. cit.*, p. 361.
[2] *Loc. cit.*, p. 516.
[3] *Loc. cit.*, p. 556.
[4] *Loc. cit.*, p. 361.
[5] Talma: Eine psychische Function d. Rückenmarks, *Pflüger's Arch.*, XXXVII, p. 621. See also the preceding section of this study.
[6] Luchsinger: Zur Theorie. d. Reflexe, *Pflüger's Arch.*, Bd. XXIII, 1880, pp. 308 ff.
[7] Maudesley: Physical Conditions of Consciousness, *Mind*, 1887, pp. 489 ff.
[8] H. R. Marshall: Consciousness and Biological Evolution, *Mind*, No. XXXV, 1896.

brain consciousness falls under the scope of introspective psychology, but logical considerations lead to a widening of the limits of consciousness and to the hypothesis that there is a certain mentality connected with each neural action which gives us consciousness of different grades under certain conditions of neural systematization."

Of the same school of thought is Lange who takes up the question where Pflüger left it, but tries to avoid his error: that of personification. Pflüger's experiment is more valuable and fundamental than Goltz's in which it was believed that the presence of consciousness in the spinal animal was disproven. He says:[1] "Let us drop personification: let us cease to seek everywhere, in the parts of the frog, thinking, feeling, acting frogs, and try instead to explain the phenomena out of simpler phenomena, *i. e.*, from reflex movements, not from the whole, the unexplained soul. Then we shall easily discover, too, that in these already so complicated sequences of sensation and movement there is afforded the beginning of an explanation of the most complicated psychological activities. This would be a path to follow up." Lange puts his faith in modes of consciousness as Lewes does. In reply to Moleschott's statement that the whole spine may be made inactive without consciousness being affected, he says:[2] "Good! But when it is concluded that decapitated creatures have no sensation and no consciousness, Moleschott overlooks that the head separated from the spine might show its consciousness in a way we can understand, but not the trunk. What sensation and what consciousness there may or may not be in the spinal centres when separated from the head we cannot possibly know. This only we can certainly assume: that this consciousness can do nothing that is not based in the mechanical conditions of the centripetal and centrifugal nerve conduction and the constitution of the centre."

But we must follow out further the direction set by Lotze. In one of his own later statements he emphasizes the subordinate position of mechanism:[3] "We cannot be surprised at the steadfastness with which the philosophy of the feelings here seeks to oppose itself as a higher view of things to the convincing representations of the mechanical view of nature. On the other hand there seems all the more necessity for an attempt to show the inocuousness of this view, which, when it forces us to sacrifice opinion that seems to be a part of our very selves, yet by what it gives back makes it possible for us to regain the satisfaction we had lost. And the more I myself have labored

[1] Lange: History of Materialism, 1892, Vol. III, p. 127.
[2] *Loc. cit.*, pp. 127-128.
[3] Lotze: Microcosmus, p. 46, Vol. I, 1885.

to prepare the way for the acceptance of the mechanical view of nature in the region of organic life—in which region this view seemed to advance more timidly than the nature of the thing required—the more do I now feel impelled to bring into prominence the other aspect which was equally near my heart during all those endeavors."

His earlier representations were mediating views and were easily interpreted as favorable to either side. "But all the same it is in such mediation alone that the true source of the life of science is to be found: not, indeed, in admitting now a fragment of the one view, and now a fragment of the other, but in showing how absolutely universal is the extent and at the same time how completely subordinate the significance of the mission which mechanism has to fulfill in the structure of the world."

Then as to the spinal soul:[1] "We may speak of a divisible soul, if we are thinking merely of the . . . . . predisposition to mental life which seems to pervade the body: but if the divided subject be supposed to be the already developed consciousness with its remembrances and experiences, and the dexterities and knowledge acquired by means of these, we could have no clear idea of what we are saying. Yet only a divisibility of the latter kind could account for the phenomena: for the capacity of acting in accordance with circumstances would be secured for the headless trunk not a whit more easily by means of an intelligence having no experience than of a purely physical mechanism as first formed. Hence there is a choice of only two views. Either we must regard the purposive character of the movements of headless cold-blooded animals as the result of an intelligence but of an intelligence not now present in the animal, but belonging to that one soul with whose seat the trunk was once in connection and from whose deliberations proceed habits of purposive actions in the central organ and continue even after all connection between it and the soul has been done away with. Or if we conclude that they must be accounted for not by echo but by the direct presence of intelligence, there is nothing to prevent us from admitting in the spinal cord a plurality of individual beings of the nature of souls, each of which might have an intelligence for itself."

In the purposive movements of spinal animals Sherrington sees nothing psychical. A mechanical explanation is all that is necessary. He says:[2] "The joints and muscles of the limb

[1] *Loc. cit.*, p. 337.
[2] Sherrington: The Spinal Animal, Med. Chir. Transacts., Vol. LXXXII, p. 466.

have been evolved contemporaneously and together in the history of the individual and the species. No muscle can therefore be thrown into action which will move the limb in any way which is an unnatural direction." And like this is Wundt's latest word:[1] "Adaptation can never be any other than a result of elementary practice processes. At the same time it is, however, a more complex process, since its essential character consists in a plurality of exercises which produce a definite purposeful total consequence."

When Pflüger's point of view is modified as it is by Lewes and Lange it can be neither proven nor disproven by introspection. On the other hand it seems difficult to account for complicated adaptations, either in spinal animals or in the lowest living creatures, on the assumption of a pure mechanism of either the first or the second construction. Wundt believes that in the lowest animals movements are accompanied by some degree of mentality. "In the lowest animals all movements possess not the character of reflexes . . . . . but the character of psychically conditioned movements." [2]

The soul, as Pflüger used the term in his discussion of its divisibility, is an abstraction. It is beyond the reach of empirical investigation. Lotze in his earlier discussions to which reference has been made herein, also had the notion of an abstract soul set over against a concrete mechanism. But as has just been pointed out, in a later discussion he admits a different definition: one which corresponds closely to modern definitions. He has told us that the soul is divisible in so far as it is a predisposition toward mental life. This looks toward the objective face of the question.

Reviewing the history of the fifty years with reference to progress in the theory of reflex action we see clearly that the main gain has been rather in the slow alteration of standpoints than in any sudden appearance of new facts wholly incompatible with older views. The spirit of the age—its unconscious metaphysics—has changed and along with it the metaphysics of the reflex. But a point has been reached at last at which, in the opinion of the writer, at least a partial solution of the question as between the views of Pflüger and Lotze is possible.

Putting away abstract considerations, we may define the soul objectively as that feature of the central nervous system in virtue of which the organism is enabled to profit by experience. This leaves entirely out of account any consideration of the

[1] Wundt: Phys. Psyc., 1902, Bd. II, p. 332.
[2] *Loc. cit.*, p. 253.

subjective side of experience which we call consciousness. Under this definition the question of a divisible soul does not involve the question of a subjective spinal consciousness.

When soul is defined objectively as above it is open to experimental observation. The question is: Can a given organism profit by experience? In other words, can it learn? or in particular can the spinal frog learn? If it can we are justified in inferring that it has such a soul as has been described. If it has not it may fairly be regarded as a machine.

THE PROBLEM OF THE EMOTIONS.

By GUSTAV SPILLER.

I.

The favorite theory of to-day concerning the nature of the emotions is probably that of Professors James and Lange, which claims, in Professor James's words, that "the genesis of an emotion is accounted for, as the arousal by an object of a lot of reflex acts which are forthwith felt" (*Principles of Psychology*, 1891, Vol. II, p. 454), or "that the bodily changes follow directly the perception of the exciting fact, and that our feeling of the same changes as they occur is the emotion" (*Ibid.*, p. 449). In the absence of these bodily changes, it is said, we should for instance, "see the bear, and judge it best to run, receive the insult and deem it right to strike, but we should not actually feel afraid or angry" (p. 450). Accordingly, the strength of the physical excitement, and that alone, measures the emotion. A rival theory is that of Prof. Irons who, especially in a recent volume, urges that an emotion is something "unanalysable and irreducible" (*A Study in the Psychology of Ethics*, 1903, p. 39), and that its essence lies in a feeling-attitude which has an outward direction, and deals with what is significant. An emotion, according to him, is a peculiar central reaction differing from pleasure-pain and from excitement, for while pleasure-pain is, he claims, passive and subjective, emotions are reactive. Similarly, excitement is not to be regarded as an invariable property of emotion; but here, unfortunately, Prof. Irons can only cite in his favor the emotions of 'cool' contempt, 'dry' admiration, and 'cold' dislike (*Ibid.*, p. 25), and one or two similar emotions, which leaves his case unconvincing, especially since it would be open for Prof. James to reply that 'cool' contempt is only cool when there is very little of it, or one might reason that these few apparent emotions are not to be regarded as true ones, considering, especially, that of the enormous number of so-called emotions only a very few can be claimed as being unaccompanied by excitement. Furthermore, is it quite correct to speak of joy, grief, and satisfaction as being generally or always reactive? All that apparently remains unchallenged of Prof. Irons's theory is, therefore, that in some sense, emotions form

a class which must not be confused with intellect, feeling, or will.

The simplicity and plausibility of Prof. James's theory make it attractive and recommend it; but one would like to feel more certain that his theory is based on a sufficiently exhaustive examination of the essential nature of the emotions on the cognitive and volitional side, and that it represents a successful attempt to define off emotion from what is not emotion, or satisfactorily determines the elements out of which an emotion as such is compounded.

There are several states which must be distinguished from emotional states. We have certain needs, such as hunger and thirst, which move us to action; but these represent impulses rather than emotions. So, too, through acting for a period along a certain line, with emotion or without, an inclination or organized tendency is gradually formed that brings about reactions which are otherwise not infrequently connected with emotional outbursts. Where such an inclination is, passingly or permanently, connected with more or less strong feeling, we speak of a sentiment or a passion rather than of an emotion. Diffused excitement may also exist without emotion. A person may be 'upset' by everything, the excitement continuing long after the occasion is past or remembered. Here we have a case of unstrung nerves or general mental irritability as distinct from pure emotion which is directly aroused by some definite circumstance and does not merely express absence of general equilibrium; perhaps we ought to speak in such a case of a secondary emotion, since the excitement aroused depends on a persisting excitement due to a preceding emotion. One form which excitement takes is that of hysteria, and here the absence of a definite end conclusively betokens the absence of emotion. We also speak of moods and temperaments, and these, too, cannot be called emotions, for they are diffused and are only minimally determined by outward circumstances. Cravings, desires, and bodily and mental pleasure-pains must likewise be excluded, though it is still to a large extent an open question how far these and other states enter into emotions or are confused with them. From the preceding it will be seen that we have not as yet even a recognized selective hypothesis as regards what states are to be termed emotions, for it is questionable, for instance, whether any diffused excitement or mood should be regarded as non-emotional. However, Prof. Irons's excellent analyses in Chapter III of his quoted work are well worth studying in this connection.

What, then, is an emotion? It might be said that if we combined Prof. Irons's "feeling-attitude" with Prof. James's "perception of bodily symptoms," which are said to mark an emo-

tion, we should have discovered the tantalizing secret of the emotions. On this hypothesis we should argue that somehow the thought that "certain circumstances are deplorable," and the connected or unconnected bodily excitement constitute together the emotion of grief, and that the absence of either of these argues the absence of that emotion. Yet in my case, and this is the burden of the present paper, introspection tends to show that these two together do not constitute an emotion, for we should only have a certain cold and colorless statement and an unconnected and unaccountable bodily excitement, and that is all. We must evidently go deeper. Everything which affects us or moves us is in some degree significant, *e. g.*, I put the pen down now because I want to think. Here there is no emotion—or shall we say an infinitesimal amount of emotion?—and in normal action, of which this is typical, emotion plays accordingly no traceable part. Let us, however, say that I have to decide whether, seeing that my life is in danger, I should stay or run. If I am unexcitable and high-principled, I deliberate unexcitedly about the matter and finally reach some conclusion as to how to act, so, if there is no time, I coolly fight for my life. Here we observe an outwardly directed and decidedly significant thought and no excitement or emotion, and yet in another individual the emotion of fear is induced very readily, even apart from exceptional significance. Similarly with bodily excitement. What physical signs are there to divide emotional from non-emotional states? The presence and the intensity of anger, of fear, and of rejoicing can be discovered and measured by the peculiarity of the physical excitement only after we are aware of the nature and the degree of the related cognitive and volitional excitement.

Now an analysis of an emotion as experienced, will, if the analysis be correct, give us some glimpses of the secret we are desiring to bring to the light. Not so long ago I was steering a boat for the first time, and as the small river, the river Charwell at Oxford, was crowded with boats, I was naturally anxious that no accident should happen through my inexperience. The emotion of anxiety was here fully developed, and I spent part of my time, whilst steering, in studying that emotion. The result of that examination was a view of the nature of the emotions which was new to me, for I had been an indifferent disciple of Prof. James. (Gustav Spiller, *The Mind of Man*, 1902, p. 271.) I noticed that I was in a state of perplexity. I could rest neither in hope nor in fear. I wanted to do more than one thing at a time. I was straining to know what to do. I wished to settle several difficulties at once, and hence a scurrying to and fro of ideas, most of which were scotched or killed before they had fully emerged. Here there was no serene

statement " I am perplexed as to what I shall do," and an unconnected or connected physical excitement; but there presented itself instead a crowd of incomplete thoughts hastening they knew not whither, and repeated and vain attempts were made at finding solutions. This state of mind, then, independently even of any bodily excitement, seems to be an emotional state. In other words, where there is no excitement directly aroused by a definite object or idea there is no emotion, and the greater the excitement the greater the emotion. At the same time the resulting bodily changes are at least frequently a substantial part of the emotion, for in acute anger and fear, where there is usually a reference to the taking of immediate action, it is clear that various physical adjustments similar in purpose to the cognitive and volitional adjustments above described—primarily muscular, and secondarily nervous and vascular—will be attempted. An angry man who would strike but restrains himself is physically very nearly in the condition of him who does strke. Or to take the case of my steering, it is evident that my muscular system was, in an incomplete way, as active as my brain. Indeed, the ordinarily accepted ideomotor theory implies that even the most ideal cognition which refers to action involves a partial carrying out of that action, and from this it follows that where the mental excitement referring to action is great, the physical excitement will be great also, and where the mental excitement is at its lowest there the physical excitement will be also at its lowest. So long, then, as the ideational content of an emotion has reference to the initiation of bodily action, so long is it impossible to regard the mental and physical excitement as separable or as independent; as well speak of an experimenter manipulating an instrument, and separate his thought from his action, without robbing both processes of their meaning. It is not a question of bodily excitement giving rise to mental excitement or mental excitement giving rise to bodily excitement, since they are both substantial parts of one act. Moreover, we have to remember that there is such a process as a mental reverberation complimentary to the physical reverberation, spoken of by Prof. James, for an important thought, or one in which we are immersed, or professional matter, ever comes uppermost, however strenuously we may endeavor to dismiss it; and likewise in an emotion, the mental reverberation, in accordance with this, lasts considerably beyond the initial moment of excitement, even after the basis for the excitement has been proved to be illusory. On Prof. James's theory, if I mistake not, the feelingless thought passes through the mind like a flash and there is an end to it; but in my emotional experience the problem suddenly raised by what provokes the emotion gives rise to a more or less tur-

bulent flow of thought which seeks, as in non-emotional experience, to meet the situation, and in proportion to the importance of the situation and the power of self-control there is produced a more or less orderly or chaotic mental condition. If only for the reason that Prof. James's theory does not seem to correspond faithfully to what we know of the normal workings of the human mind under the provocations which call out an emotional response, his theory does not appeal to the judgment as being readily defensible as regards its fundamental outlines. To this must be added that the power of suppressing certain emotions by inducing dissimilar bodily emotional attitudes, referred to by Prof. James, is paralleled by the well known fact that we can perhaps even more readily suppress emotions by turning our thoughts away into different or more desirable channels, and that certain classes of eccentricity and madness are consistent with the absence of any obvious emotional states. Our conclusion, then, must be that Prof. James is right in urging that physical excitement is present in emotion as an essential constituent; only we must hold that emotion also implies mental excitement, a more or less broken or hurried stream of thought tending to mental and bodily chaos when the excitement is great, and that these two series constitute properly only one series.

We have examined a pronounced form of the emotion of anxiety, a cousin to fear; but an analysis of any other emotion, say that of fear, will yield the same result. In unmistakable fear, for example, we are mentally overpowered; we try to think of ways of escape; we doubt whether we shall succeed; the thoughts hurry along wildly; we tend in many directions both as to thought and action; we are irresolute; we are in a highly strained condition as regards muscles and nerves, no sooner initiating movements than inhibiting them. Naturally, therefore, the body generally is thrown into a state of excitement, since muscles, nerves and blood vessels, as well as brain, are strongly affected, unless, indeed, the emotion is of a contemplative or minimal kind, though even here most probably we have no exception.

An emotion, of course, is not bound to find its expression in words, and close observation both of men and animals bears this out. To take a homely illustration from the farmyard, an escaped pig, vainly attempting to elude its pursuers when cornered in a chase, shows all the signs of fear which we might observe in a conscious human being, and the organized nature of all thought would point to the same conclusion. Sometimes, it is true, bodily excitement is out of proportion to the normal mental excitement (and *vice versa);* but here, owing to a morbid or other condition, the bodily excitement, and, indirectly

the mental, are over-emphasized and approach the stage of chaos. Where the emotion does not find its normal expression, there we speak accordingly of morbid fear or hope. However, it is quite within the range of possibility that now the bodily and now the mental excitement should be the more prominent.

This, then, seems to be the nature of an emotion. A certain more or less urgent need, aroused directly by some definite object or idea, is eagerly and yet vainly seeking to be satisfied. In so far as there is, as a consequence, mental excitement, a more or less turbulent endeavor to meet the situation in a satisfactory manner, so far the state becomes emotional, and the proportionate absence of this peculiar mental excitement argues the proportionate absence of an emotional coloring of new attitude. Such a definition excludes inclination, sentiment, passion, secondary emotions, mental and physical prostration and derangement, cool and deliberate judgment, habit, pleasure-pain, craving, desire, objectless excitement, temperament and moods, and seems to embrace nothing but pure emotional states and everything which characterizes these states. The definition likewise includes all pleasant emotional states such as rejoicing, for all emotions. like all pleasure-pain, argue a disturbance or absence of complete satisfaction. It explains also the origin in excitement of any and every attitude called an emotion and the necessary presence of physical excitement—even to the point of morbidity—when an emotion is observable, though it is somewhat difficult to reconcile this explanation with Prof. James's view which asserts that the perception itself, say that of a white sheet, immediately produces physical changes, the feeling or perception of which changes is supposed to be the emotion. Prof. Irons's 'cool' contempt also receives its explanation, for many attitudes are only minimally emotional and others are only by courtesy called emotionals. Besides, contempt may reach the stage of disgust, loathing and nausea, and be anything but free from excitement, while, on the other hand, our contemptuous attitude towards some one may merely imply that we have no very exalted opinion of him. In proportion as a state of mind becomes directly excited through a definite object or idea, so may we speak of the presence of emotion, and as the ideational stream takes different directions, so we speak of different emotions. Emotions, therefore, do not appear to be something unanalyzable, nor are they composed of a serene thought and pleasure-pain or bodily excitement; but in their essence they represent mental excitement produced directly through a definite object or idea, naturally combined with physical excitement, expressing the fused cognitive, volitional and active aspects. An emotion, consequently, is only a mental attitude in a state of excitement, and has no separate or independent existence.

II.

While there is no limit to the states called emotional, except that of general and almost infinitely varying interests, some of the emotions are yet well defined and discharge themselves along instinctive lines of activity or bring instinctive relief from unrest. Naturally, too, certain emotions are more fundamental than others, and those which are so, such as anger and fear, are common to most of the higher animals and their young and have their lines of development and discharge determined hereditarily.

The first thing we may learn, therefore, from an ethical standpoint is that emotions, or at all events pronounced emotions, are not indispensable in civilized life, for since an emotion represents but mental excitement, it lies within the range of possibility to avoid that excitement, *i. e.*, while desiring, thinking and acting are essential to life, *e. g.*, to the life even of the uncompromising Stoic, emotions are not equally indispensable. We may, therefore, ask the question how far emotions are ethically justifiable.

As to the master emotions their case is the most evident from a moral point of view. In animals generally, and also in the earlier stages of human development, it is essential that reaction should, as a rule, be instantaneous and appropriate, for the capacity for reflection exists then but to the slightest degree and adaptations of a far-reaching kind are, therefore, impossible, especially considering that adjustments relating to the far future are inappropriate and useless in a primitive environment. Certain practical attitudes, associated namely with marked excitement, anger, fear, revenge, jealousy, are accordingly developed to protect the primitive individual automatically. The place of reflection is here occupied by an inherited reflex mechanism, and though the consequent reactions of such a mechanism would be necessarily inappropriate in the extreme within a complex society, they suffice for primitive beings primitively placed. With high mental development the readily acting reflex mechanism does not disappear; but the need for it has almost passed. The civilized person's relation to the world around him is complicated, and owing also to his being a part of a highly evolved community, he has no need to react quasi-mechanically. He may allow trained impulses to do thoroughly what fitful anger, fear or jealousy would otherwise most imperfectly accomplish. When, for instance, some one is wronged, instead of being angry, he may address himself to some appropriate impulse in his would-be enemy, calmly prevent him from acting unrighteously, or appeal to the law.

A highly advanced being adjusts his actions in accordance

with a general ideal, that is, in accommodating himself to the present, he takes into account the future. His aim is not to deal out rough justice, but strict justice. Excitement, he knows, generally emphasizes the needs of the moment at the expense of those of futurity, and accordingly he is averse to acting when in anger or in passion, unless he has first weighed the matter. An emotion suggests, as Bain has pointed out, a more or less self-centred idea, and tends, as a consequence, to interpret everything from its own point of view. The advanced ethical man, therefore, will proceed, prompted by powerful and trained sentiments which have primarily respect for universal well-being, and will not act on the basis of sudden emotions, except of such as have been aroused as the result of examining a case in a judicial spirit.

To live according to broad principles is the tendency of highly developed beings. This implies that we ought not to be governed by suddenly upwelling emotions, but rather by sentiments of a most comprehensive type, and that without anger, fear, indignation, hatred, contempt, disdain, envy, jealousy, malice, pride, arrogance and shame, one may pass an eminently innocent and useful life. The judge may be just and severe in his judgments without in any way being either unemotional or unduly influenced by emotions out of accord with the general justice of the case. Our sense of right and truth, our systematized conception of life, our respect for all sides of our nature and for the common good, the inclination towards living a consistent life, and broad-based sentiments, are guides which replace rather than supplement the primitive emotions.

Like and dislike, delight and sorrow, love of truth, justice and beauty, might be said to be the minimum for man in the way of emotions, if, indeed, any of these here named can be regarded as emotions, for the first few are perhaps pleasure-pain attitudes, and the latter perhaps only refers to sentiments or inclinations. We are, for instance, glad that our son is a good scholar (without being proud of him), that a certain person is near us (without loving him), and that we are able to protect ourselves (without being revengeful). We can dispense with by far the greater number of emotions and sentiments.

Though many emotions, and especially the primitive ones, become superfluous or of little account with social advance, yet it appears difficult to imagine that men, however advanced, should not *pity* him who suffers, or that we should not warmly *love* our offspring. It is well to remember that the only thinkable and defensible ground on which men can agree for opposing a course of thought or action is its incompatibility with the total individual and social life, and that any other standard

demolishes itself. Consequently, in so far as an emotion or indeed any practical attitude deleteriously affects life as a whole, so far is it to be suppressed, while when an emotion proves to be helpful, its cultivation is morally imperative. The abrupt, irresponsible, blinding emotions, such as rage or terror, may be safely discouraged, for the love of a comprehensive ideal can replace them; but the tender emotions, such as sympathy, pity, and kindliness, need only be guarded against when they tend to become unjust to life as an organized complex. The advanced ethical man, if this be true, will be endowed with emotions; but he will not permit them by their impulsiveness to endanger or nullify the rights of the other parts of his nature. Indeed, heartiness and boisterousness are probably more satisfying and argue a richer and robuster nature than stiffness and immovability, and, therefore, ethically speaking, we ought not to aim at the total suppression of the emotions, but rather at limiting them to their legitimate sphere of influence. Absence of impulse, desire and emotion do not accordingly distinguish our ideal of a man; for he who only acts from a sense of duty and has no love for his kind stirring in his breast, is scarcely a human being at all, leaving aside the implied neglect of the additional stimulus to right conduct which, as Bishop Butler has pointed out, is supplied by the affections.

Again, if we are to lead a useful life, mental excitement cannot be avoided altogether, for sometimes we have to decide hurriedly between alternatives, *e. g.*, as to whether we are to catch the train now about to depart or go by some later one, in which case there is no room for calm deliberation, and especially is this so since we must recognize an emotional state even when the perturbation is very feeble.

Many of the so-called emotions are really not emotions at all, though the precise border between excitement and absence of excitement is as difficult to ascertain as the frontier between excitement and mental and physical confusion or prostration. We sometimes say "I *fear* he will come," "I am *sorry* for him," "I *pity* his lot," "I *dislike* him," "I am *angry* and *indignant*," when we mean "I have reason to think that evil will result from his coming," "I should have saved him trouble if it had been in my power," "I would improve his lot if I could," "I am not attracted by him," "I am not favorably impressed." In these instances there is but the veriest atom of excitement, and thus, with ethical advance, the vocabulary of emotion is deprived of a large part of its meaning and becomes almost wholly metaphorical, that is most of the emotions lose nearly all their intensity. If the Stoic, as Marcus Aurelius Antoninus describes him, or the Cynic, as Epictetus draws him, represents the most advanced ethical type, then the ideal man is

ever serene and gentle, and never unjust by reason of his emotions. Such, too, has been "the ideal of ancient sages," for Confucius, Buddha, Socrates, Jesus, Marcus Aurelius, all typify serenity, and the phrase "an excitable saint" approaches to a contradiction in terms. And yet it is difficult to determine the golden mean which shall do justice to human nature as such. Is it not rather that, instead of being coldly intellectual, the advanced ethical man is full of fire and vivacity, and that he only differs from those who rank below him in never having evil or superfluous emotions such as rage or pride, and that his emotions never make him act unfairly to himself or to his fellows? The above view follows that of Prof. James, who says that a non-emotional existence, "although it seems to have been the ideal of ancient sages, is too apathetic to be keenly sought after by those born after the revival of the worship of sensibility, a few generations ago" (p. 453). Modern ethics will have nothing to do with an ascetic view of life or a pessimistic theory of human nature. Let the petty emotions and the mischievous ones and most of the primitive ones go or be applied only under primitive conditions; let the gentle emotions, the useful ones and the great ones be encouraged; and let the emotions have no unsettling influence over our conception of life as a consistent unity. The result will be a being completely capable of self-control and never requiring much of such control, gentle in manner, self-possessed, stern when necessary, and bubbling over with the joy of life and action. Such a being would be free from pride, arrogance, humility, envy, jealousy, ambitiousness, anger, hatred, scorn, contempt, and all the related brood of semi-civilized and petty emotional states, sentiments, and practical attitudes. His heart, on the contrary, would overflow with sociability and pity, with a warm desire for doing justice and defeating injustice, and with a love of truth, beauty, goodness and nature.

III.

On the psychological side the conclusions arrived at in this paper as regards the nature of the emotions are far from claiming that they close or exhaust the inquiry. While insisting that physical together with cognitive and volitional excitement, directly induced by some definite object or idea, appear as invariable concomitants in an emotion and that an emotion is a mental attitude in a state of excitement, we have left undefined the problem of the exact nature of the excitement, as in anger and fear, in rejoicing and grieving. The term excitement, in other words, but vaguely hints at the passive factors involved in an emotion which tends, in the one direction, to a cold inclination, and, in the other, to mental and physical

chaos. Secondly, we have attempted, by the method of exclusion, to separate strictly emotional from non-emotional states, and we excluded, on the ground of absence of excitement directly aroused by a definite object or idea, Inclination, Habit, Sentiment, Passion, Secondary Emotions, Temperament, Mood, Diffused Excitement, Desire, Craving and Impulse. Still, the enormous number of so-called emotions, bewildering in their variety and relationships, requires to be explained and to be reduced to order, and, as Prof. Irons well shows in the volume alluded to, the list of the emotions demands to be reconstructed. Here we can only suggest that since the circumstances which arouse emotions give rise to an infinite variety of ways of grappling with them, there is no limit to the possible variety in emotions and classes of emotions. To this must be added that the emotions as they were conceived of before the era of scientific analysis, are largely misinterpreted through the almost invariable presence in them of countless non-emotional elements. Perhaps, too, most states, if not all, have an emotional aspect, and are as yet not recognized as such because the excitement is relatively feeble and is, therefore, overlooked. Suffice it to have drawn attention to the importance of doing greater justice to the careful analysis of the cognitive and volitional elements in emotions as they are experienced. It may be, after all, that not until the more general truths of normal psychology are well established, shall we be quite clear as to what is and what is not to be included in emotional states and what are their fundamental characteristics. Perhaps we shall find then to our surprise, as we have already endeavored to show, that there are strictly speaking no emotions, and that what we call emotions are directly aroused attitudes in a state of excitement. We should, on this principle, understand at once the great multiplicity of the so-called emotions and the variety of explanations concerning them.

The problem does not seem so complicated on the ethical side. That the primitive emotions, such as anger and fear, partake of the nature of instincts is as evident as that with the growth of civilization the violence, the reflex character, and the imperial sway of these emotions are steadily discouraged, and that in the end there remain a few master sentiments, appropriate to the new conditions—sociability, respect for self and others, pity, and love of doing justice to the whole nature of man. We recognize here three stages: the primary and instinctive one, where there are a few headstrong emotions which only take directly into account the good of the moment; a transitional and semi-anarchic period, where an army of emotional and other attitudes, such as pride, ambition, contempt, shame, conceit and love of glory develop; and a final and or-

dered stage, where a few impulses, principles, emotions and more especially sentiments of a comprehensive character, harmoniously seek to do justice to the individual, to society, and to nature, as an interdependent whole. The intellectualist who dreams of a golden age when men will despise emotions and sentiments and be wholly guided by reason, meets with no support from the above analysis; nor does the moralist find favor who exalts the sense of duty and the possession of principles and despises the affections and the joy of life; nor again are those encouraged who plead for the so-called artistic temperament which would perpetuate the present stage of rule by innumerable petty emotions. Speaking generally, primitive virility, if one may prophesy, will remain, while a few broad-based and powerful sentiments will take the place of the few imperious emotions which dominate primitive life.

A SIMPLE COMPLICATION PENDULUM FOR QUALITATIVE WORK.

H. C. STEVENS, Cornell University.

An experiment is described in Titchener's Laboratory Manual, i, S. M., 115, to demonstrate the law of attention that "the process attended to rises more quickly than other processes which enter consciousness simultaneously with it." The apparatus required is a bell metronome. The two simultaneous stimuli are the click of the metronome and the sound of the bell.

Under favorable conditions, the experiment may be successfully performed. The observer, by attending mainly to the click or mainly to the bell, is able to alter the apparent temporal relation of the two impressions. There are, however, two general objections to the method. In the first place, the fact that the two impressions belong to the same sense department is a real difficulty for novices,—sometimes even for experienced observers who have not had special practice. A second objection is that the succession of clicks and tones tends to fall into a subjective rhythm, in which now the noise and now the bell-sound may be accented. This accentuation gives the accented member of the series a greater hold over the attention, so that the verification of the law may be helped or hindered in a way not allowed for by the experiment.

A simple device for the simultaneous presentation of two impressions from different sense departments can be made with a bell metronome and a piece of cardboard. The cardboard is cut in the form of an arc of a circle whose radius is the length of the metronome pendulum. Scale divisions of 5° are laid off on the circumference. The cardboard arc, with the 0° of the scale corresponding to the position of equilibrium of the pendulum, is impaled on the eye which serves to lock the lid of the metronome. In this position, the white cardboard, bearing its scale, forms a background, in front of which the pendulum oscillates. A piece of red paper, cut in the shape of an arrow-head, may be spitted on the pendulum to make its movements more conspicuous. The metronome should be set to beat about 72 times in the 1 min. with the bell ringing at every complete oscillation of the pendulum. One has, by this arrangement, the simultaneous presentation of a visual and an auditory impression. The position of objective coincidence can be found, approximately, by slowly moving the pendulum with the hand, until the bell sounds. With this apparatus the experiment is performed by directing the attention, in the one case, mainly to the red arrow-head, and, in the other, mainly to the bell, the experimenter reading off from the scale the position of the arrow in each case. Objective simultaneity, for the metronome used in the Cornell Laboratory, came at 22°. When I attended mainly to the arrow, the bell seemed to ring at 30°; when I attended mainly to the bell, the arrow was between 10° and 15°. Similar results were obtained by other observers.

This experiment, of course, inverts the conditions of the 'complication experiment,' in which nothing is said about the voluntary shift of attention. But inasmuch as the rate of the metronome pendulum can be varied, the apparatus could be used for qualitative complication work.

LITERATURE.

PSYCHIATRY.

By Dr. I. H. Coriat.

SOME RECENT LITERATURE ON TRAUMATIC INSANITY.

The growing need for the determination of exact etiological factors in the production of various mental disorders, has been the impetus for the recent literature and observations on traumatic insanity. These psychoses are polymorphous in type and comprise delirious and dreamy states, various vaso-motor disturbances, irritability with special intolerance for alcohol, peculiar amnesias and marked memory disorders, deterioration, marked tendency to fabrications, paranoic and katatonic states; or the trauma may be a contributory factor in the production of other so-called organic and functional psychoses, which we are in the habit of looking upon as having a different etiology. Viedenz (Ueber psychische Störungen nach Schädelverletzungen, Archiv für Psychiatrie, Bb. 36, 1903) reports four cases, and concludes that mental disorders following trauma are protean in type, but have the following common traits, viz.: change of character, excitability, diminution of memory and intolerance for alcohol. The most frequent psychoses are primary dementia, katatonia, and confusional and stuporous states. Paranoia is rare. The first case relates to that of a soldier, in whom, following a fall from a horse, there arose a state of apathetic dementia. In the second case the patient became excited after a fall from a tree, and then there supervened a katatonic complex, with catalepsy, negativism and convulsions. The third observation is that of a boy, at 14, whose grandfather died of alcoholic insanity. Following a fall on the occiput, there developed attacks of ambulatory automatism. In the fourth case, trauma was probably only an exciting factor. A general, at 40, fell from a horse, and three months after the accident there developed symptoms of general paralysis, with speech disorder and repeated apoplectiform attacks. Death ensued after fourteen months and the anatomical picture bore out the clinical diagnosis.

Kalberlah (Ueber die Acute Commotionspsychose, zugleich ein Beitrag zur Aetiologie des Korsakow'schen Symptomencomplexes—Archiv für Psychiatrie—Bb. 38, H. 2, 1904) contributes an important paper showing the immediate bearing of trauma on Korsakow's symptom-complex. The symptoms described by the Russian psychiatrist, as being practically a clinical entity and occurring almost wholly in the subjects of chronic alcoholism, and almost invariably associated with neuritis, have during later years been found to accompany a multitude of other conditions. It may occur in chronic alcoholics with or without neuritis ("polyneuritic mental disorder"—Cole), in acute melancholia with anthrax infection (Soukhanoff and Tscheltzoff), following pelvic abscesses from extra uterine pregnancy, in delirium tremens, presbyophrenia, in cases with unknown etiology without neuritis, in chronic lead intoxication and cerebral syphilis, in general paralysis and following infection and strangulation. Meyer and

Raecke (Zur Lehre von Korsakow'schen Symptomen-Complex—Archiv für Psychiatrie, Bb. 37, 1903) have recently reported the symptom as occurring in three alcoholic cases without neuritis, one senile case with gross vascular lesions, and in one case of sarcoma of the frontal lobe.

Considering the multiple etiological factors concerned in the production of this symptom-complex, a paper like Kalberlah's, in which the clinical history of even one case, is reported in great detail, is of exceptional value, for the greater part of mental disturbances following trauma are of a slow development, and are usually given meagre detail in connection with medico-legal complications. Kaplan's "traumatic degeneration of character, an explosive diathesis," merely relates to the impulsive acts and insidious changes of character in these patients. According to Kalberlah, there may be marked mental enfeeblement, lessened memory capacity for the reproduction of recent impressions (Merkfähigkeit), disorders of attention (Aufmerksamkeit), perception and judgment, a variable emotional condition and increasing apathy. There may also follow transitory unconscious states and various motor, sensory and vaso-motor disorders. He finds all the elements of the traumatic psychosis in chronic alcoholism, and calls the mental symptoms immediately following injury "the acute psychosis of commotion." The cases which he studies are given in great detail. The first case was that of a mason, 43 years of age, non-alcoholic. In ascending a frail ladder, the structure broke, and he was thrown to the ground, a distance of five meters, striking his head on a stone. He bled profusely and was picked np unconscious. In the second case, there was a fall from the height of four meters, the injury being to the head. The first case showed focal symptoms in the shape of a double facial paralysis, and in both the typical Korsakow's psychosis developed; disorder of attention, euphoria, absence of hallucinations and of any fixed delusions, and a prominent defect of orientation. The memory was particularly affected, there being amnesia, a persistence of old memories and a marked enfeeblement for recent memory impressions.

E. Meyer's contribution (Korsakow'sche, Symptomencomplex nach Gehirnerschütterung; Neurologisches Centralblatt—Bb. 23, N. 2, 15. Aug. 1, 1904) relates to the case of a fireman, aet. 32, with a negative history of alcohol, heredity and infection, who suffered a severe injury of the skull, with probably a fracture of the base. This was followed by coma; then there supervened a state of agitation and deep disorientation, with a high grade memory defect for recent impressions, fabrications and amnesia for the accident. There was no ataxia or neuritic pains, the knee-jerks were always present, but there was severe headache and dizziness. Later there occurred two epileptic seizures. An operation revealed nothing and no improvement followed. The mental picture in every way resembled Korsakow's symptom-complex. The paper gives a comparative table of the patient's reaction to the same series of questions at different intervals, in order to show the contradictory replies, the result of disorientation and the memory disorder.

Amongst our clinical material we have seen a case in many respects similar to those above. The patient was a weaver, moderately alcoholic, who fell from an upper story balcony, striking on his head. He was picked up unconscious and in what was thought to be a dying condition. There was deviation of both eyeballs to the right, but no bleeding from the ears or nose. On regaining consciousness, there was marked confusion, disorientation, dizziness especially on turning quickly, deviation of tongue to the left, impairment of smell

in both nostrils, marked memory defect for recent events and impressions, rather prominent florid fabrications, and amnesia for the accident, but nothing of the retrograde or retroactive type.

Dr. Adolf Meyer's paper (American Journal of Insanity, January, 1904) relates to a larger clinical material than any of the previous contributions. In all, 31 cases are reported, and the histories show how multiple may be the psychic symptoms following trauma. The literature and the various theories relating to the exact bearing of trauma on mental disturbance, are reviewed. He suggests that in this group of cases there are at least five directions in which clinical studies may be of value.

1. Rieger's attempt at an inventory of the patient's mental possibilities.
2. The study of the vaso-motor neurosis of Friedmann.
3. The examination for the "explosive diathesis" of Kaplan.
4. The occurrence of dazed and dreamy states, conditions of anxiety, epileptic seizures.
5. The establishment of the etiological factors.

The prominent types in the classification are suggested as follows: the direct post-traumatic delirious states, the post-traumatic constitution, the traumatic defective conditions, traumatic psychoses from injury not directly affecting the head, and psychoses in which trauma is merely a contributory factor, such as general paralysis, manic-depressive insanity, and katatonic and paranoic states.

Archives of Neurology from the Pathological Laboratory of the London County Asylums. Edited by F. W. Mott, Vol. II, 1903. Macmillan & Co.

This is the second annual publication of the work of the pathological laboratory of the London County Asylums, edited by Dr. F. W. Mott. It is twice as voluminous as the publication of the previous year, and a glance at the titles shows how valuable are the works of Dr. Mott and his associates in the domain of neurology and psychiatry. The first and most voluminous paper, comprising 327 pages, on "Tabes in Asylum and Hospital Practice" is from the pen of the editor, and it is certainly the most complete and masterly exposition of the subject which we have met. The paper is especially strong on the physical symptoms of tabes, including minute studies of the sensory disturbances, and of the morbid anatomy and pathology of the disease. The clinical material is large, comprising in all seventy-four cases, but it is unfortunate that the mental states accompanying tabes and tabo-paralysis are not given in more detail, considering the excellence of the status of the physical conditions and the anatomical findings. Dr. Mott takes a strong stand regarding the syphilitic origin, if not nature, of tabes, general paralysis and the combined types of the disease, and this is in harmony with our modern conceptions in spite of a few weak arguments to the contrary. His words are worth quoting and most convincing: "That we cannot prove more than 70 or 80 per cent. of tabic and paralytic patients to have suffered with syphilis, is no argument against the doctrine that both paralysis and tabes are post-syphilitic affections. . . . In sixty cases of syphilitic brain disease I could not obtain a history in more than 70 or 80 per cent. The very important experiments of Jadassohn and Hirschl, that only in one-half of the cases of undoubted severe syphilis is it possible to prove primary infection, and the statement of Lang that in one-third of the cases of tertiary syphilis the primary infection was not demonstrable, are arguments against those who will not be convinced that syphilis is the essential cause of tabes and general paralysis unless it can be proved in every case."

He divides his cases into six groups as follows:

1st group—Those cases of tabes which present some unusual clinical phenomenon, or are adapted for explanation of some essential feature of the disease.

2nd group—Cases of tabes with insanity, not essentially the combined form with general paralysis, but with any fundamental psychosis, usually of the nature of an acute hallucinosis.

3rd group—Tabo-paralysis.

4th group—Tabo-paralysis with marked speech affection.

5th group—Optic atrophy and Tabo-paralysis.

6th group—Conjugal Tabes and Paralysis.

The next three papers discuss at length "Amentia" (Idiocy and imbecility) by A. F. Tredgold, the "Histological Basis of Amentia and Dementia" by Joseph Shaw Bolton, and the "Pathology and Morbid Anatomy of Juvenile General Paralysis" by George A. Watson. The papers deal in so minute a detail of the various subjects of which they treat, that only meagre justice could be done them in the limits imposed by a review. "The Prevention of Dysentery in the London County Asylums" by Dr. Mott, is a paper giving practical suggestions on the clinical symptoms of the different types of the disease, its prevention, its bacteriology and morbid anatomy.

There are two contributions on nerve chemistry, one by Halliburton and Mott on the coagulation temperature of cell globulin and its bearing on hyperpyrexia, the other upon the cholin test for active degeneration in the central nervous system. The first paper details a series of experiments on cats and on human brain material confirming the hypothesis that the physico-chemical cause of death from hyperpyrexia is due to the coagulation of the cell globulin; and anatomically this is coincident with the chromatolytic changes in the nerve cells. The paper on cholin gives a modification of Halliburton's recent method, but here it is applied to human blood and not to cerebrospinal fluid. The presence of the cholin crystals in the form of the double platinum salt, is a measure of myelin degeneration, but the test is useless to determine whether a cause is organic or functioned, unless the organic disease is active at the time the blood is drawn.

Smith's paper is a psychopathological study of the range of immediate association and memory in normal and pathological individuals. The importance of testing patients subject to mental disease in regard to their power of acquiring new impressions and ideas, and in their capacity for reproducing the same has been greatly emphasized of late, especially by Kraepelin and his pupils in their publications in the Psychologische Arbeiten. The tests were done orally, and uniformity in the presentation of the series was secured by means of the metronome, the pronunciation of each letter being made to coincide with a beat of the instrument. As the result of a large series of experiments, he draws the following conclusions: The method of immediate oral reproduction of auditory impressions appears to be reliable and practicable both with normal and pathological individuals, and to be well fitted to test the range and character of immediate association in different mental states. He divides these cases into three pathological groups: (1) three men in the early stages of general paralysis; (2) general paralysis in three men and one woman but the disease is less marked than in the first group. The male cases had coincident tabes and there was a history of alcoholic excesses; (3) three women and two men presenting general features of some degree of dementia, confusion and loss of memory, with a history of previous marked alcoholism. With normal subjects the range of immediate memory has usually a definite limit which varies with each individual, but as a

rule is found to lie at five letters. When the limit is reached the addition of one letter to the series of auditory impressions produces a very decided fall in the number of series which are reproduced quite correctly; this fall amounts to 40 to 50 per cent. on the average. With abnormal subjects the relations are similar but less clear. In the abnormal cases the general nature of the pathological changes are similar. They consist in a marked diminution in the power of reproducing impressions in correct order, and in a more or less distinct increase of all the errors indicating the more severe forms of associational disorder. In all the groups (normal and abnormal) within certain limits the total of errors indicating partial dissociation or disorder remains relatively constant and reaches approximately the same absolute magnitude. None of the groups shows any marked fluctuations of attention, or any distinct and regular improvement by practice. With the help of this method we are able to differentiate with some precision between the more permanent memory and the power of immediate reproduction.

Bolton's papər on the "Pathological Changes in the Medulla Oblongata in Acute Diphtheritic Toxæmia" is a study of the degenerative changes caused by the diphtheria toxine in the nuclei of the medulla. The contribution by Cole is a further histological study of one of the three cases published in Brain (Autumn, 1902), relating to changes in the central nervous system in alcoholic paralysis with the characteristic polyneuritic mental disorder.

A Case of Moral Insanity with Repeated Homicides and Incendiarism and Late Development of Illusions. H. R. STEDMAN. Boston Medical and Surgical Journal, July 21, 1904. Vol. CLI, No. 13.

This very excellent contribution relates to the case of the moral imbecil, J. T., who suddenly became notorious as one of the greatest poisoners of modern times. The entire history is given in great detail showing the gradual development of the moral weakness, the repeated homicides by poison, the medico-legal complications and the metal status of her hospital residence with the elaboration into a fixed, delusional state. There follows a discussion of the case by members of the medical society before whom the paper was read. On the whole, it is an important contribution to the much-discussed subject of moral insanity and it will certainly repay perusal.

The Localizing Diagnostic Significance of so-called Hemianopic Hallucinations with Remarks on Bitemporal Scintillating Scotomata. A. PICK. American Journal of the Medical Sciences. Jan., 1904. Vol. CXXVII, No. 1.

Recently Prof. A. Pick, of Prague, has made several contributions to American and English Medical Journals, and the one whose title appears above is probably the most important, combining as it does localizing neurological diagnosis, with the most prominent symptom occurring in the course of mental diseases. He bases his paper on a recent observation of Jolly, that light phenomena in a hemianopic field of vision, have their origin in the primary optical tracts and not in the cortex. He elaborates on this statement, however, by showing that more complicated phenomena than simple, elementary light sensations, in fact, real hallucinations, may be produced in an hemianopic area by focal organic or functional disorders in the optic tracts. Four cases are given, mostly from the author's private practice. In regard to visual hallucinations in general, it is now known that they may take place in any part of the visual apparatus, from the cornea to the cortical layer in the occipital lobe, and the author promulgates a

similar theory regarding hemianopic hallucinations. With these latter there usually coexists other focal symptoms, such a paraphasia, paragraphia, hemianæsthesia, etc. He argues against the functional nature of the disturbance, basing his theory on the clinical material at hand. These hallucinations may be mere light phantasms, color disorders with changing of colors, animals, vague figures and phantoms, colored lights and planes, and lines and figures moving in snake-like gyrations. Sometimes the figures are sharply defined, at others they may preseut a hazy "fringe."

Ueber Ermüdungskurven bei Gesunden, Neurosen und Psychosen. DR. BREUKINK. Monat. f. Psychiatrie und Neurologie. Bd. XV. H. 4, April, 1904.

The work was done in the psychological laboratory of the psychiatrical clinics of Utrecht and Halle. The ergograph of Kraepelin was used with weights of 5, 4 and 3 kilograms. The lifting was carried on to exhaustion with a pause of two minutes between each series of curves, and the rhythm was registered by the metronome. In the illustrations the following ergograms are given: hysteria without any particular motor symptoms, neurasthenia, multiple sclerosis with one-sided hemi-paresis (the curve being taken with the left hand), chorea minor almost entirely confined to the left side with a curve of a permanent contraction in the same patient, and two curves from a case of dystrophia muscularis progressiva. The author draws the following conclusions. In healthy individuals there is ordinarily found a high convexity in the commencement of the curve. In many cases of hysteria, which psychically showed only the well known psychopathic constitution, there is often found a remarkable giving out which soon passes away, so that in these cases, after the pause of two minutes there may be the same or even a greater number of liftings than before the pause. In most cases of neurasthesia, the course of the height of the exhaustion curve was straight or concave. In chorae the involuntary contraction soon strengthened, but then weakened, so that the curve had a very irregular fall. The retardation of the fatigue was also remarkable. In cases of organic cerebral hemi-paresis (multiple sclerosis, brain thrombosis, general paralysis) the number of the liftings and the mechanical work of the paralyzed side is less, while the average lift height is about the same as on the healthy side. In a case of dystrophia muscularis progressiva, it was observed on the contrary that with a small average lift height, the number of liftings was disproportionately large.

[For further applications of the ergograph to psychiatry see "Ueber die Wirkung der Theebestandtheile auf körperliche und geistige Arbeit," by A. Hoch and F. Kraepelin, Psychologische Arbeiten, Bb. I, H. 2 and 3, where it is applied to the mental and physical effects of caffein and the ethereal oils of tea; also "On Certain Studies with the Ergograph," by A. Hoch, Journal of Nervous and Mental Diseases, Vol. XXVIII, p. 620, 1902, where there are given graphic studies of retardation and inhibition on the voluntary motor processes in the depressed phase of manic-depressive insanity, and finally the same writer's review of psychological and physiological experiments done in connection with mental diseases, Psychological Bulletin, Vol. I, Nos. 7-8, June 15, 1904.]

Vergleichende Psychiatrie. F. KRAEPELIN. Centralblatt für Nervenheilkunde und Psychiatrie. Bb. XV. July, 1904.

This highly suggestive paper, is we believe, the first serious contribution to comparative psychiatry. The observations were made in the

insane asylum of Buitenzorg in Java, under the direction of Dr. Hoffman. It discusses the important question, whether individual and racial peculiarities and habits, as well as the sociological status of patients, have any visible bearing upon their mental disturbances. In the study of the cases, we must look, not upon a temporary symptom-complex, but upon the complete disease picture from its earliest development and along the entire line of its evolution. There were no cases of alcoholism in the native born, while fifty Europeans in the asylum were strongly alcoholic. Opium delirium is seldom seen among the Chinese, while malarial psychoses are quite rare. Epilepsy occurred eight times among the native born, and twice in Europeans. In 370 native patients there was not one case of general paralysis or brain syphilis, whereas among the fifty Europeans in the same asylum there were eight cases of general paralysis. Dementia præcox was more prevalent among the natives, but the course of katatonia, when it occurred was very mild. Manic-depressive insanity was seldom met with, and its different phases were less prominent than among Europeans. Delusions when present were weak and almost never systematized, while auditory hallucinations were rare, this probably being due to the fact that thinking was carried on without the aid of auditory images.

The two diseases most prevalent among the Malays are Amok and Látah. The latter, which is probably not a clinical entity, shows automatisms with coprolalia and clouding of the consciousness. Many of its manifestations are hysteriform in nature, whereas the blind fury of Amok seems to be related on the one hand to katatonia, and on the other to psychic epilepsy.

[Both folk psychology and comparative psychiatry in connection with individual and racial peculiarities are becoming of great importance in the study of mental disturbances. Among the more primitive peoples, with an undeveloped and non-flexible language, it would be interesting to note how far sound associations enter into the productions of the manic phases of manic-depressive insanity. Also the modern developments of science would seem to furnish a fruitful field for the study of the genesis and content of hallucinations and delusions, and especially the frequent explanation of the latter as being based on electrical influence or telephonic or phonographic communication. Negroes seem especially prone to a blind, religious exaltation, both as individuals and by psychic contagion in their revivals and camp meetings. This exaltation may comprise the entire mental disturbance, or it may be merely an episode in a fundamental psychosis. In katatonia in Russian Hebrews, the religious coloring is derived from Old Testament conceptions of Jehovah or from later rabbinical lore and superstitions. Refusal of food among them has often a religious and not a delusional basis. The few Chinese patients that we have observed, have shown religious ideas that were at once a combination of Christianity and the teachings of Confucianism. The native language of patients has frequently a great influence in the production of their auditory hallucinations, for, in English speaking patients of foreign birth, the imaginary voices are nearly always in their mother tongue. Urban districts are almost entirely free from syphilis and consequently from general paralysis. The occurrence of pellagrous insanity in Italy and of hasheesh delirium in Egypt and India are also well known examples, while the long continued solitary meditation of the mediæval mystics, with the irbeatific visions, and of the Hindoo priests may also have a bearing on the production of certain psychoses, if indeed they are not the mental disturbances themselves.]

Les Obsessions et la Psychasthenie. Vol. I, par Pierre Janet; Vol. II, par Pierre Janet et F. Raymond. Paris, Felix Alcan, 1903.

This recent monumental work of Janet offers interesting and valuable problems to the psychologist, psychiatrist and general student of medicine. H. N. Gardiner, of Smith College, has given a critical review from the standpoint of pure psychology, and it seems opportune at the present moment that an analysis be forthcoming according to the tenets of psychiatry, for the work deals so exclusively with the borderland cases of mental disturbance. The psychasthenias are related on the one hand to the broad clinical complexes of hysteria and epilepsy and on the other to the fundamental psychoses. This type of borderland case is of increasing and paramount interest to the alienist, and a study so complete as is contained in these volumes, will bring forth points that will serve to more clearly elucidate these important problems. The second of these volumes is written in conjunction with Prof. Raymond, and in all there are extended clinical records of 236 cases, comprising the entire range of obsessions, impulses, mental manias, folies, tics, agitations, phobias and algias, the different délires, the conditions of anxiety and distress, feelings of inadequacy, the peculiar feelings of strangeness and depersonalization, and the neurasthenic states, all of which have fundamental characteristics and are grouped under the title of psychasthenias. The analytical details relate principally to symptomatology and the evolution of the disease-processes.

Out of his clinical material, the author selects five cases, which he has studied with great care and detail. The other cases are grouped around these five which serve as types. These five cases, which must be read carefully before the text can be fully comprehended, are case 222, Obsessions of sacrilege and of disgrace of body, with erotic hallucinations, tic des efforts and feeling of incompleteness. Case 223, Obsession of contract with the devil, with numbness and feeling of inadequacy. Case 157, Hypochondriacal obsessions, masturbation, phobias, symbolic hallucinations, Case 166, Délire de maigreur in reaction to ideas of disgrace of the body, aboulia, crises of agitation and refusal of food. Case 171, Crises of sleep, feelings of inadequacy, disgrace of self, gastric atony, arthritis.

The first group to which the author turns his attention are the obsessions. These are intellectual phenomena of the highest order, an idea, and often, an idea which is very abstract and complicated. They are distinguished by their absence of usefulness in practical life; in fact, they are pathological and not normal ideas. They are divided into five classes; obsessions of sacrilege, crime, disgrace of body, disgrace of self and hypochondriacal ideas. These again have numerous subdivisions which are taken up in detail. In spite of their variation and multiplicity of symptoms, this group has common characteristics; thought is always directed to bad behavior, it relates to objects of the external world, to the feelings, or to the body of the patient himself. Extremes of behavior and reaction are always reached. There is always a strong tendency to action, but a very marked absence of execution, yet certain patients will perform acts having some relation to the obsessions, or even contrary acts may be the result of the dominating idea. In these obsessions the idea is stronger than the motor impulse, in other words there is always a coincident weakness of will. As these states endure for years and are unaffected by any method of treatment, by psychotheraphy or otherwise, the condition of these patients is truly pitiful, for the permanent fixation of their ideas is very prominent and interferes with all their habits of daily life. The associated hallucinations are always vague, the visual image seems to be without

color and the words without sound; they have not the characteristic of exteriority, they lack reality, they are merely symbolic of the dominating idea. Even when these patients exert the stronger will power to keep away from these obsessions, yet frequently by the association of ideas, it is brought irresistibly back to them.

These obsessed patients show other phenomena, for, without any determinant idea, they are forced to think in an exaggerated manner, their head "works" in spite of them, they feel compelled to accomplish useless movements and have violent, irresistible emotions. These ideas may exist without any obsessions and they then constitute a group of symptoms more simple than the obsessions. This group is called the forced agitations, which are divided into the mental agitations, relating to interrogation, hesitation, deliberation, precision, symbolism, calculation, research, perfection, infinity, compensation, day dreams and feelings relative to time, such as the past and future. The motor agitations are the tics and the emotional agitations which comprise the states of anxiety, the phobias and algias. The tics are the *diffuse* motor agitations, the phobias and algias belong to the same group, but are more systematized. All the phobias have a disagreeable character, a certain vague, yet deeply rooted fear, while the algias, on the contrary, actually give a suffering of somatic pain in the performance of certain daily acts. Both of these have fundamental and common characteristics, they are developed as the result of sensations excited in certain parts of the body; probably in a region already hyperæsthetic. The states of anxiety are studied both from the standpoint of psychological analysis and the accompanying physiological phenomena, such as cardiac disorders, disorders of the respiration and intestinal tract, vertigo, various paræsthesias, nocturnal terrors and profuse perspiration. The crises of these forced agitations always begin at the occasion of a voluntary act, and with these are associated profound disorders of attention and varying emotional states. Conscience always remains clear during the crisis, but there is an irresistibility to the agitations and a feeling of satisfaction in their accomplishment. If there should arise a feeling of resistance, there always accompanies this more or less acute mental or physical pain until the act is accomplished.

The stigmata of these psychasthenic states are next studied and given minute analysis. These diverse stigmata are the sense of incompleteness in the motor, intellectual and perceptual fields, the symptoms of the narrowing of the field of consciousness as in hysteria, namely the anæsthesias, subconscious movements and the hypnotic sleep and finally the disorders of the will, intelligence and emotions. Certain abnormal physiological conditions invariably seem to accompany these phenomena, such as has already been mentioned under the states of anxiety. These multiple phenomena are, according to the author, the result of the "lowering of the psychological tension," just as hysteria was to him "a narrowing of the field of consciousness." In the evolution of the disease processes there are many factors as in all so-called functional disturbances, such as heredity, age, sex and certain physical states and normal conditions. The onset of the disease may also be variable on account of the protean symptomatology, while the course may be either acute, chronic, intermittent or in episodes. The treatment is both suggestive and medicinal and is directed to tonic and sedative medication, waking suggestion, re-education and "relief of the psychological tension."

Les Lois Morbides De L'Association Des Idées, Par M. PELLETIER, Paris, 1904, Jules Rousset.

The first chapter of this book deals in detail with the laws of the

association of ideas in normal states and the author adopts as a working basis the subdivision of resemblance, contiguity, contrast and repetition. The symptoms of mania as then discussed, both as a phase of manic-depressive insanity and as a syndrome in other mental states, such as general paralysis and in certain cases of dementia præcox. Then follows a discussion of the incoherence and flight of ideas in mania. The fifth chapter is devoted to an analysis of the manic phase of eight cases of manic-depressive insanity, according to the conversational method, in order to clearly demonstrate the flight of ideas and the disturbances of association. It appears to us, that for this latter purpose, the test tables of Sommer would be preferable. These disturbances are studied according to a subdivision of sound, contrast, contiguity (either clear or vague), resemblance and repetition. The latter part of the book is devoted to an exposition of the association of ideas in the mentally weak, with experiments on the reaction time. The laws of the association of ideas do not vary in that they are fundamental. Whatever be the mode of psychic activity, the laws of resemblance, of contrast, of contiguity and of systematic association are always at the base. In mental disturbances, and notably in mania and states of mental weakness, the laws are absolutely the same as in normal conditions and act in the same manner. The loss of the higher faculties, the reduction and enfeeblement of attention, the plethora of ideas, are not alone able to explain these symptoms of mania. Both mania and states of mental weakness are the consequence of a very great enfeeblement of the psychic processes. The enfeeblement is not supreme, the thinking processes are the same as in normal states, only they are more defective. Psychological analysis shows that this enfeeblement is in the state of consciousness itself, and a simple difference of force modifies the course of the state of consciousness, in such a way that the result is either a cohesion of language or the extreme incoherence of mania.

Recherches sur la Structure de la Partie Fibrillaire de Cellules Nerveuses a L'État Normal et Pathologique. Par G. MARINESCO. Revue Neurologique, 15 Mar., 1904.

This paper is an exhaustive study of the neuro-fibrillar structure of nerve cells, as revealed by the new method of Ramon y Cajal, namely, treatment of the tissue with silver nitrate and reduction with pyrogallic acid or hydroquinon. There are twenty-five illustrations, which show in an excellent manner, the distribution of the neuro-fibrils within the cell body, in both normal and pathological states. After treatment by the method detailed, the motor cells of the cord are of a brownish-red color with a bunching together of the fibrils into a net-work which takes the same color. This net-work is best seen and is most complete in the cords of newly born rabbits. In the base of the cone of origin the neuro-fibrils diverge and spread out in a fan-like manner; they then come together and form filaments which lose their fibrillary appearance. In the motor cells, the net-work of neuro-fibrils is more dense than in the cells of the nuclei of the motor cranial nerves, but even these latter do not present uniformly the same structure. In other cells the fibrils form a dense network around the nucleus. The direction and disposition of the fibrils follow the form and volume of the cell, but in fusiform, oblong and triangular cells, the fibrils traverse the cells without forming a network. The individual neuro-fibrils have not the same uniform dimensions, and in the triangular and oblong cells they have a fasciculated or striated appearance. Marinesco distinguishes two classes of network; the superficial or perisomatic, and the deep or peri-nuclear, and these latter

again have primary filaments which follow a long course in the cytoplasm and secondary short filaments which take a transverse direction. Some of the long fibrils have a serpentine arrangement. In experimental anæmia and rabies and also in secondary lesions after injury of the peripheral nerves, the fibrils diminish in number, become of a pale diffuse brownish color with a fine granular appearance, or may show fusiform thickenings along their course. These pathological appearances vary according to the intensity of the injury or toxine, or the duration of any of the abnormal conditions.

The Psychological Bulletin. (Literary Section of the Psychological Review, June 15, 1904.)

This number of the Psychological Bulletin is a happy innovation; it is entirely devoted to psychiatry and neurology and is produced under the editorship of Dr. Adolf Meyer, who writes the first paper in the number, devoted to the exposition of modern clinical psychiatry, especially as exemplified by the evolution of the recent German schools, under the leadership of Kraeplin, Wernicke and Ziehen. The second paper is a review by Dr. August Hoch of psychological and physiological tests made in connection with the study of various mental diseases, both from the standpoint of research and diagnosis. The review of psychological literature that follows consists mainly of abstracts of recent important books and papers bearing on neurology and psychiatry. Among these are the recent volumes by Bethe and Nisal on the anatomy of the nervous system, the Archives of Neurology of the London County Asylums, and finally minute abstracts of Bleuler's case of one-sided occupation delirium in a general paralytic, and Liepmann's remarkable case of one-sided apraxia, which has recently come to autopsy. The other reviews relate principally to current German, French and Spanish literature and comprise such contributions as Köster, Saint-Paul, Berze, Klippel, Ramon y Cajal and Dejerine.

Der tic, sein wesen und seine Behandlung. Nebst eine vorrede von PROFESSOR BRISSAUD. DR. HENRY MEIGE und DR. E. FEINDEL. Deutsche Autorisierte Ausgabe von Dr. O. Giese. pp. i-xii, 1-398. Leipzig und Wien, 1903.

The mental and motor disturbance designated by the title of this book has long been of considerable interest to psychiatrists and practicing physicians. But its general characteristics, and especially the conditions and factors in its development should be of equal interest to the student of general psychology. The authors have been for a number of years special students of their subject, and have not only a thorough acquaintance with the work of their predecessors, but have themselves contributed more to it than any other two workers in the field.

Their object in the present book is (1) to present clinical material that is itself of interest, and (2) to differentiate clearly tic from the numerous other forms of motor disturbance. The scope of their data and discussions, however, is wider than this statement of their purpose. The clinical material is presented in the form of copious illustrations taken from the histories of cases. To this attaches, in the reviewer's judgment, fully half the interest and value of the book, but it cannot be presented in a brief review. A history of a case, the "prototype of a tic patient," constitutes an introductory chapter and gives a general picture of the characteristics of tic.

The following chapter outlines the results of predecessors. The

main deficiency in previous and current conceptions of tic lies in the misunderstanding of the relation of its motor and mental aspects. It is a psycho-motor phenomenon, but has been regarded as entirely psychic or entirely motor alone. Again, some who have not overlooked the motor aspect have included other motor processes, or have limited tic to its clonic form, excluding the tonic contractions that have all the other characteristics of it.

The nature of the motor process in tic is described in several following sections in connection with other motor processes, a special comparison being made between tic and spasm. The authors recognize two main forms of motor reactions. (1) The simple spinal reflex, with no co-ordination, and no systematic function. In the origin of this the will has no part, and can but seldom repress it. (2) The functional movements. These are co-ordinated, purposive movements, more or less automatically performed. Here belong (a) the movements of the vegetative functions, circulation, digestion, etc. (b) Movements that co-ordinate definite functions from birth onward in the origin of which the will does not participate, but over which it may develop an influence, *e. g.*, breathing, sucking, etc. (c) Movements which arrive at a complete co-ordination only after more or less learning, *e. g.*, walking, chewing. (d) Movements which originally attach themselves to ideas, but which through practice also develop the automatic character of the functional activity. Implicitly, the movement performed with voluntarily directed effort would constitute an intermediate class. The spinal or bulbular reflex movement is a spasm, if its stimulus has a pathological cause. Tic belongs to the second group, that of the functional movements. The functional movement involving voluntary muscles is a tic, if the motor reaction shows quite definite pathological signs.

The nature of the mental characteristics in tic are described mainly in connection with the consideration of the origin of the pathological motor process. Tic may take its origin in an external stimulus, in imitation, or in a centrally aroused idea. There may be the desire to avoid an unpleasant stimulus and hence the consequent movements, *e. g.*, turning of head to the side because of a rough-edged collar, winking because of dust particle under the eye-lid. Or there may be the desire to reproduce a certain stimulus, *e. g.*, pulling of a joint in order to hear it snap. A striking attitude or performance of another may be imitated because of its peculiarity. Finally, the idea of a certain movement and the impulse to perform it may be a pure product of the imagination. In any of these cases, and the special instances under each are varied and numerous, repetition makes the movement habitual. The original peripheral stimulus or centrally aroused idea is absent, but the performance of the movement continues without occasion or purpose, as an habitual automatic activity. In the development of this condition of the now pathological motor process a deficient will power is an essential and fundamental element. The tic patient has a weak will, and with it may go other traits characteristic of a partially arrested mental development. The development of the pathological phenomenon takes its own course because there is not the ability of voluntary resistance. This is one of the main things that distinguishes tic from the habitual, stereotyped performance of the normal person. The tic patient cannot check his habit, and further, he suffers through its repression. Also, the habitual movement of the normal person occurs unawares, when his attention is engaged in something else. Tic occurs when the mind and body are unoccupied. It weakens or disappears when the attention of the patient is directed to some other activity.

The localization of the tic is various. Any muscle of the body may be involved. The only rule to be noted is that those muscles that are used most, or are most expressive of mental states are most frequently the seat of this pathological activity. A single muscle alone is never involved, but only such groups as are used in co-ordinated functional movements. The authors devote a section to the description of different tics, naming them according to both the group of muscles involved, and the function of those muscles, such as the facial tics, or the mimetic tics, the lip tics, or the sucking tics, etc. Copious illustrations are given as usual. The form of the motor process may be that of either the clonic or tonic contraction. Other authors have not included the latter, but there are tonic contractions that have associated with them all the other characteristics of tic. There is no regular rhythm or frequency in the occurrence of the movements. They make their first appearance most frequently during childhood, but may begin at any other time of life, and usually continue up to old age. Several sections are devoted to the relation of tic to other diseases, differential diagnosis, other symptoms associated with tic, etc., that are of interest more to the clinician than to the psychologist.

The general outcome of these considerations is that tic is a unique psycho-motor phenomenon that may have various symptoms of other nervous disorders associated with it, but it bears no necessary relation to these. As to the etiology of tic nothing very definite is ventured. A variety of things may be the initiating occasion of what will later develop into a tic, but these cannot be regarded as causes. After these have been enumerated, the authors conclude that the psychic predisposition of the individual remains the *conditio sine qua non* for the origin and development of tic. The previous statement in regard to the weak will power, and partially arrested development of the tic patient is of course to be remembered in this connection. Some anatomical changes have been reported, but these the authors regard, not as characteristic of tic, but as results of complications of the cases reported. They regard tic as an inherited functional anomaly associated with a deficient development of cortical association tracts, or sub-cortical branchings, with molecular malformations that cannot be detected by present methods.

The last sixty pages of the book are devoted to a consideration of the treatment of tic. The degree of curability is, in general, dependent upon the degree of will power of the patient. Medicinal treatment is of little value. The rules of proper diet, and of general hygiene are to be observed. Proper psycho-motor training is the only direct method that can improve the condition of the tic patient. Two kinds of exercises are to be employed, together. (1) Exercises in remaining absolutely motionless, beginning with short periods that are to be gradually lengthened. (2) Regulated gymnastics, using only correct movements. Special stress is laid upon the use of the mirror. The patient is to perform these exercises also alone in front of a mirror, where he can himself see and correct his anomalies in positions and movements. Such a demonstration of his anomalies is more forceful than description or exhortation.

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Begaiement et Autres Maladies Fonctionelles de la Parole, by DR. CHERVIN. Société d'Editions Scientifiques. Paris, 1901, pp. 551.

This is a revised and enlarged edition of the author's former work. The book is divided into four parts and an appendix. In part I he discusses the rôle of speech in society and classifies speech defects according to their causes. In part II he takes up stuttering proper giv-

ing first a brief history of stuttering and then the etymology of the word "stutter," tracing it through many languages and dialects. This is followed by a valuable chapter on statistics, based on the military statistics on stuttering and other nervous diseases in France, Switzerland, England, Austro-Hungary, Belgium, Italy and Russia, and presents ethnographic maps of all of these countries except England, showing the distribution of the troubles in question. In the next chapter (VI) he takes up the definitions of stuttering, causes, age of appearance, professional stammering, sex, difficulties of respiration, verbal phobias, and hysterical stuttering. When he holds that the ratio of male to female stutterers is 10:1 he deals probably with adults. With children the ratio is probably 2:1 or 3:1. He objects to the term *hysterical stuttering* used by many writers, since speech defects due to hysteria, according to him, are not stuttering; he would prefer the term "speech defects of hysteria."

The next four chapters are devoted to methods of treatment. We first have a short history of methods in general, followed by one of the Chervin method. According to the latter method the treatment is divided into two main parts, which, however, are usually carried on together, namely, the functional and mental. The first consists of respiratory and articulatory exercises, and the second part in insisting vigorously that the patient carefully imitate the instructor. The length of the course is three weeks. During the first week the patient is taught the elements of speech and methods of breathing, and he must keep absolute silence outside of instruction hours. During the second week the principles of respiration and articulation are further practiced, the patient is given liberty of speech, but must speak slowly and methodically. During the third week attention is given to phrases and to inflection of the voice, gradually coming to natural diction and ordinary conversation. When, at the end of three weeks, the patient is dismissed, he is considered only convalescent; he should practice his exercises from 2-3 hours a day for at least a month. The last chapter of part II is devoted to a discussion of the question of military exemption on account of stuttering.

Part III deals with various other functional speech defects. It is divided into four chapters. In the first he discusses various forms of stammering *(blésité)* and says the sex ratio is the inverse of that in stuttering. In the next chapters he takes up rhotacism—faulty pronunciation of the letter *r*, the eunuch voice and nasalizing. Faulty articulation he cures by an orthophonic treatment. If nasalizing, however, is due to organic causes a surgical operation should precede the didactic treatment.

In part IV the author deals with speech defects due to organic causes, such as hare-lips and cleft palates.

In the appendices the first eighty-nine pages are devoted to some old traditions and some collections from folklore relative to speech and speech defects; and finally the last sixty-two pages are given to reports from official commissions, medical and scientific societies, setting forth the merits of the Chervin method—advertising material which should not have been made an integral part of the book.

In a book coming from a physician and teacher of such experience, one is disappointed not to find more clinical data. The anthropological portion of the book is very good and the extensive statistics should be helpful. The book is written in a clear and concise style, and in spite of its defects is one of the best works on the subject which it treats.

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